

*D. Bennett  
with D. Charlton best regards*

(29)

THE RETROSPECTIVE ADDRESS,  
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DELIVERED AT

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*HELD AT SHEFFIELD,*

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Two opposite courses present themselves for our selection upon the present occasion of addressing you. The one, easier for ourselves, by far, would have been to extract from various sources the most interesting cases and observations; the other, a much more arduous task, is to analyze the general opinions of the profession on each disease, to amalgamate them with our own experience, and, thus condensed, to present them to our hearers. It is needless to say that we have preferred the latter and the more difficult alternative; how far we may have succeeded it will be for you to decide.

We regret much that it has seemed necessary to the Council of the Association to re-unite the vast subject of physiology with that of practical medicine, as the obvious result has been, that much interesting matter relating to both departments has in this year's Address been omitted. On this account we cannot occupy your time by any preliminary observations, though the now agitated questions of Medical Reform, of Hygiène, and of

Medical Education, present tempting subjects whereon to discourse. One point, however, we may briefly notice,—the question of the preliminary education requisite for a medical man. As we advance in practice and in years, we must all be sensible of the immense advantages conferred on the practitioner, by his having previously secured a firm basis of classical and general learning. The man who, relying solely on his medical knowledge, ventures to mingle in the great struggle of the world, will sooner or later find himself so situated as that, with all his tact, he cannot avoid exposing his ignorance before those whose good opinion he is most anxious to conciliate. It is with pain that we have observed of late an increasing disposition to undervalue, as preparatory to our profession, a sound and complete classical education. We grant that the study of the Greek and Latin authors has been too exclusively pursued in this country; but we will ever maintain that a thorough acquaintance with the great writers of antiquity is essential in forming that taste, in fashioning that elegance and purity of expression, which is so important an ingredient in the success of every medical man. The study of the modern languages is now becoming more general in this country; and of these we need only say, that every additional tongue that is acquired, opens up new fields of instruction, and mingles a vast amount of intellectual gratification amid the more earnest duties of our profession. With these few observations we proceed to the more immediate subjects of

### PHYSIOLOGY AND ANATOMY.

We shall not here, any more than at the beginning, apologise for the magnitude of the subject, for no one could expect that even the most condensed notice of all the investigations in physiology, or of all the microscopic researches in anatomy, could by any means be recorded in this Address. This is the more to be regretted in regard to physiology; for the great minuteness of detail necessary for the clear explanation of many recent doctrines and discoveries, has rendered several of the most valuable essays and papers almost incapable of analysis. We shall follow here that arrangement of the different portions of our subject which has been generally adopted in former years.

## NERVOUS SYSTEM.

Longet and Matteucci\* have continued their experiments upon the nervous system, and with some interesting results. Up to the present time, they observe, experimentalists, when acting by electricity on the nerves, have always found that the muscles to which they ranified, contracted both upon the formation and the interruption of the Galvanic circle, and that this took place whether the current was direct or inverse. But after some time these contractions are found only to ensue on the establishment of the direct, and the interruption of the inverse current. These experiments, however, have been made specially on the lumbar and sciatic nerves, which, as is well known, are of a mixed character, containing both sensitive and motor fibres. But they find the results to be totally different in regard to purely motor nerves, as the anterior fasciculi of the spinal marrow; for in experimenting on them, the contractions only ensue at the commencement of the inverse or centrifugal and the interruption of the direct currents. By acting on the posterior fasciculi before they are separated from the rest of the cord, convulsions are manifested only at the interruption of the Galvanic circle, in whichever direction the current of electricity be made to flow, and these movements are evidently occasioned by a reflex action upon the anterior roots of the nerves, as they cease entirely when the latter are divided. Finally, our authors here quoted have never observed the grey matter of the spinal cord to exercise any influence upon either motion or sensation, as they have repeatedly removed the whole of it without any visible effects upon the anterior or posterior columns.

Volkmann and Bidder's experiments prove that the sympathetic nervous system is essentially different from, and independent of, the cerebro-spinal system in the discharge of its functions. They removed the brain, or the spinal cord, or both, from several frogs, leaving in all cases the medulla oblongata to continue the respiratory movements. The general result was to show the strongest contrast between the effects of this destruction on the parts supplied by cerebro-spinal nerves and on those supplied by

\* *Archives Générales de Médecine*, October, 1844, p. 251.

the sympathetic. In the former case all the muscles were rendered at once incapable of contracting upon either voluntary or reflex stimulus; in the latter instance, it was long before any such loss of power was manifested.\*

Henle and Kölliker have observed a peculiar mode of termination of the nerve fibres in the little bodies, which are found chiefly upon the cutaneous nerves of the hands and feet, and to which has been given the name of Pacinian corpuscles. The nerve fibrils are seen to terminate in a knob, or else in a bifurcation within these bodies, and never in loops; nor does the terminal enlargement of the fibril resemble a ganglion corpuscle.†

We can only here refer to the excellent paper by Dr. Laycock, of York,‡ on the "Reflex Functions of the Brain," and to the essay of Dr. W. F. Barlow,§ of Oxford, upon "Reflex Action in Paralysed Limbs." M. Bourgery has also published an interesting paper on the "Splanchnic Nerves." The principal object of Dr. Laycock's paper is to confirm his formerly expressed opinion, that the brain, although the organ of consciousness, is equally subject to the laws of reflex action with the other nervous ganglia of the body; and to his arguments, in illustration of the theory, we beg to call especial attention, though to give even the briefest analysis of them here would be far beyond our limits.

It is impossible here also to detail the numerous observations recently made upon the functions of individual nerves; but we consider the conclusions of Mr. Paget in this respect to be of great value. Mr. Paget does not believe that the precise functions of the individual nerves have yet been determined, but that most probably:—

1. The glosso-pharyngeal is chiefly the nerve of the sense of taste, and in a less degree a nerve of common sensation.
2. That the glosso-pharyngeal is, according to the experiments of Müller and Hein, the motor nerve of the stylo-pharyngeus, and probably also of the palato-glossus. The branches which it gives to the digastricus, stylo-hyoideus, and constrictors of the pharynx,

\* *Paget's Report; British and Foreign Medical Review*, April, 1845.

† *Paget's Report*, 1845.

‡ *British and Foreign Medical Review*, January, 1845, p. 298.

§ *London Medical Gazette*, 1845.

appear to be sensitive, or else derived from the facial and accessory nerves, with which it has previously united.

3. The pneumo-gastrie is from its origin composed both of sensitive and of motor fibres; but it cannot be decided at present whether it alone supplies any particular muscles, or whether in all its muscular branches, and especially in those given off above the œsophageal, there are filaments from the accessory as well as from its own roots.

4. The accessory nerve contains in all its lower roots motor fibres alone; in its upper roots it is not improbable there are some sensitive fibres also. It is a motor nerve of the sterno-mastoid and trapezius muscles, and it is very probable that it gives, by its internal branch and other communications, motor fibres to the pneumo-gastric, from which they are subsequently distributed to some or all of the muscles of the larynx and pharynx, and in some animals to muscles of the palate.

### RESPIRATORY SYSTEM.

M. Rochoux\* calculates that the number of the cells in the lungs amounts to 600,000,000, and that about 18,000 are grouped around each bronchial termination. The lamellæ by which these cells are formed are composed of loose filaments, and in the angles resulting from the intersection of these lamellæ, ramify the capillary blood-vessels. The observations of Mr. Rainey† are nearly in accordance with those of M. Roehoux.

A memoir was read during the course of the last year to the French Academy, by M. Natalis Guillot, on the "Carbonaceous Matter produced in the Human Lungs, both in adult and in advanced age." This matter appears to be carbon in an exceedingly minute state of division; and wherever it exists to the thickness of more than a millimetre, the veins, arteries, and air-tubes and cells are there obliterated, and such altered portions of the lungs are no longer liable to inflammation, though such may arise in the vicinity from excessive accumulation of the carbonaceous product. When the carbon is collected around tubercular deposits, the latter do not soften; the disease is then arrested in its progress, and the tubercles become calcareous and gritty.

\* *Gazette Médicale de Paris*, January 4, 1845.

† *London Medical Gazette*, April 4, 1845.

M. Vierordt has ascertained that the quantity of carbonic acid expired is much less when the respiration is accelerated than when it is performed slowly.

A valuable paper upon the "Nature and Causes of Asphyxia" has been written by Mr. Erichsen,\* of London. The author believes the cause of the stoppage of the circulation in asphyxia to be three-fold, and that it depends:—

1. Upon the arrest of the respiratory movements.
2. Upon the weakening of the heart's action by the circulation through its substance of blood deprived of its stimulating qualities, and also diminished in quantity.
3. Upon the obstruction offered to the passage of venous blood through the lungs. This is caused by the refusal of the minute pulmonary veins to receive venous blood; and he considers venous blood to act in this case as a positive stimulant to the arterial system, though a sedative action has generally been attributed to it.

The observations of Mr. Erichsen, excellent in every way, deserve a longer notice; and his conclusions regarding the proper treatment of asphyxiated individuals will, we hope, produce some important alterations in the present management of cases of apparent death by asphyxia.

Although Mr. Hutchinson's essay on "Vital Statistics"† would seem to belong to another division of this Address, still the important results he has announced are so intimately connected with respiration as to demand a place here. Mr. Hutchinson has found that the capacity of the chest for air depends upon the height of the individual, and that for every additional inch of height, from five to six feet, eight additional cubic inches of air are given out by a forced expiration. This important fact has been proved by experiments, carefully conducted, upon 1,200 different individuals; and Mr. Hutchinson has since extended his observations to the chest in the diseased state, and especially in phthisis. In all cases he found his former conclusions pleasingly corroborated, the amount of capacity for air being constantly diminished, but always in a corresponding ratio with the height of the individual. Thus it was found that in a man of six feet,

\* *Edinburgh Medical and Surgical Journal*, January, 1845, p. 1.

† *Journal of the Statistical Society*, London, August, 1844.

though proved by stethoscopic examination to be far advanced in phthisis, the capacity of the lungs was still greater than in a man several inches shorter, and whose lungs were but slightly affected.

The power of the muscles of respiration forms also an important part of Mr. Hutchinson's essay, and he believes that by a careful comparison of the capacity with the power alluded to, many valuable data relative to the lungs in the diseased state may be elicited. Mr. Hutchinson has constructed instruments for measuring the force of the respiration, as well as for ascertaining the capacity of the lungs. He divides the respiratory power into that of inspiration and expiration, the latter, of course, being the higher of the two, though he prefers the inspiratory power whereon to form his tables, on account of its greater regularity, and of its being less liable to be altered by habit or by various occupations. From the very ingenious tables which this writer has constructed, we find that in selecting men for any physical duty, five feet seven inches, or five feet eight inches, should be the standard for power, as in those above the latter height the force of respiration diminishes, though the capacity of the lungs continues to increase. Mr. Hutchinson thinks that we may suspect disease in the lungs whenever the expiratory power is not one-third stronger than the inspiratory.

## CIRCULATION.

*Blood.*—The excellent Gulstonian lectures on the “Composition and General Physiology of the Blood,” have deservedly added to the name which Dr. G. O. Rees had previously obtained as an accurate and conscientious observer. In these recently published lectures we meet with some novel views. After referring to the admirable researches of Dutrochet on endosmose and exosmose, Dr. Rees points out the application of these doctrines to the changes undergone by the corpuscles of the blood. He states that it may now be regarded as nearly certain that the corpuscles possess a vesicular structure, and also that the fluid within these is red in colour and the containing membrane white; and that the inclosed fluid must be of the same specific gravity as that in which the corpuscle floats. It is well known that the blood-corpuscles collapse and become flaccid when placed in contact with a solution of a higher specific

gravity than the liquor sanguinis, and become distended and much rounded under opposite circumstances. Dr. Rees believes that the fibrin of the blood is dissolved, and not suspended, in the liquor sanguinis, from the circumstance that if the latter were the case the corpuscles would be rapidly collapsed by solutions of 1.050 specific gravity, inasmuch as the serum suspending the fibrin could only have a specific gravity of 1.029 to 1.030, and the corpuscles would necessarily contain a fluid of no higher specific gravity than this.

Dr. Rees does not doubt that the red corpuscles contain nuclei, equal to about two-thirds of their size, and that the whole of the iron of the blood is derived from the serum of the chyle, and is contained in the red corpuscles. The chyle being of lower specific gravity than the liquor sanguinis, will naturally, according to the principles established by Dutrochet, part with its constituents to the corpuscles, and thus the latter are supplied with iron.

With regard to the fibrinous corpuscles, Dr. Rees proposes a most ingenious theory to explain the fact that these bodies, though larger than the blood-corpuscles, are exuded under certain circumstances, in preference to the latter. The blood-corpuscles he considers to resemble bladders filled with an incompressible fluid; the fibrinous corpuscles are solid, but sponge-like bodies, much more likely to pass through orifices of less than their own diameter.

Dr. Rees believes the blood-corpuscles to multiply by division, and, finally, he opposes the theories both of Mulder and of Liebig, in regard to the change of venous into arterial blood in the lungs. Mulder insists that the distinction of colour between arterial and venous blood, depends solely on a physical difference in the surface of the corpuscle, which forms a semi-opaque concave mirror in the former case, and a more transparent convex body in the latter; moreover, that during respiration the colouring matter of the blood undergoes no change, and indeed plays no part, either as a whole, or in regard to the iron, which exists therein merely as a simple element. Dr. Rees, on the contrary, suspects the hæmatosine to be the principal agent in the change effected; but the mode in which this is accomplished is still to us a mystery.\*

We have not space for Mr. Gulliver's researches on "The Formation of the Buffy Coat of the Blood," nor yet for the briefest

\* *London Medical Gazette*, 1845.

† *Medical Times*, February 22, 1845, p. 453.

analysis of Dr. Calvert Holland's interesting essay on the moving powers of that fluid.

Mr. Addison,\* of Malvern, suspects that the fluid which distils from the capillary tufts of the Malpighian bodies of the kidney, is the same as that in which the blood-corpuscles have floated in the living body, and that this fluid is liable to constant variation, according to the qualities of the liquids swallowed or the nature of the chyle that is elaborated. The urine, then, of a healthy individual is a much nearer approach to the fluid in which the cells float, in their circulation through the living body, than either the serum remaining after the fibrillation of a mass of blood, or the liquor sanguinis which sometimes floats upon its surface. Mr. Addison concludes that the liquor sanguinis, the colourless and plastic layer at the surface of buffy blood, is analogous to the stationary, colourless, and plastic layer seen in the irritated vessels of the living frog, and very different indeed from the fluid forming the vehicle by which the cells circulate through the living body, which latter he considers to be a saline, limpid, and variable liquid, of an excrementitious rather than of a nutritive nature.

Mr. J. H. Walsh, of Worcester, has revived, under an improved form, the old doctrine which ascribed a sucking as well as a propelling power to the heart. He compares the pericardium to a firmly fixed tent or box, which does not yield with the contraction of the ventricles and auricles, and therefore that on their being emptied, a rush of blood must necessarily take place to supply the vacuum which would otherwise ensue between the contracted heart and the firmly fixed pericardium. But we fear that the pericardium will not be found to be the firm, unyielding case that Mr. Walsh supposes.†

A most elaborate and important essay on the composition of the blood has been published by Becquerel and Rodier. Their extensive experiments tend to confirm the opinion that the fibrin of the blood is increased proportionally in inflammation; that the proportion of the globules of the blood is diminished in anaemia, after prolonged abstinence or excessive haemorrhage from any cause

\* *Provincial Medical and Surgical Journal*, August, 1844, p. 266.

† *Provincial Medical and Surgical Journal*, February 5, 1845.

whatever; and, lastly, that the fibrin of the blood is little, if at all, diminished by venesection.

On the other hand, their researches tend to invalidate the following opinions:—

1. The proportion of  $\frac{127}{1000}$  allotted to the globules in healthy blood, which our authors state is too low; nor is it the same in women as in men, being in the former  $\frac{141}{1000}$ , and in the latter only  $\frac{127}{1000}$ .

2. The received proportion of the fibrin  $\frac{3}{1000}$ , which they affirm to be too high, and give, as the proportion they have obtained,  $\frac{2}{1000}$ .

3. The pretended increase of the globules in the plethoric condition of the system; for they have found that the composition of the blood is not altered in that state, but that in plethora there is a positive increase of the whole mass of the blood.\*

*Pulse.*—M. Martin Solon has observed a venous pulse in the veins on the back of the hand, in two patients who had lost much blood by venesection. He explains this appearance by supposing that the blood had become abnormally fluid, and consequently passed more easily through the capillaries, retaining a portion of its arterial impulse. But the experiments of Poiseuille and Magendie have proved, that the more watery the blood becomes, the greater difficulty does it experience in making its way through the capillaries.†

Mr. T. W. King believes an unusually slow pulse to be caused by a very free safety-valve apparatus in the heart, (viz., a very easy means of reflux for the blood from the right ventricle,) along with retarded respiration, and that the latter is probably the result of lesion of the respiratory tracts or centres, either in the way of motor impulse or of excitatory impressibility.‡

*Heart's Action.*—Some curious observations on the action of the heart, as seen in a case of congenital deficiency of the anterior parietes of the chest, are recorded by Dr. Mitchell.§ The child

\* *Gazette Médicale de Paris*, Nos. 50 and 51, 1844.

† *Bulletin de l'Académie*, November, 1844.

‡ *Lancet*, March 22, 1845, p. 321.

§ *Dublin Journal of Medical Science*, November, 1844, p. 240.

survived its birth two hours, and the liver, the heart, the intestines, and the spleen, lay external to the ribs. Dr. Mitchell thinks, with Dr. Corrigan, that the impulse of the heart is not caused by the tilting up of the apex against the ribs, but that, like other muscles, the heart shortens and becomes thicker on the systole of the ventricle, by which the sides of these cavities are bulged out and perform the stroke. With Dr. McDonnell this writer agrees, that the complete contractions of the heart in the foetus in utero, are, perhaps, slower than those of the maternal heart. The numerous and rapid pulsations audible by the stethoscope, are owing to more sounds (auricular) being heard in the foetal than in the adult heart, and from the two auricles not contracting simultaneously. The opinion of Dr. Williams and of Mr. Carlisle, that the ventricles never become entirely empty of blood, is fully confirmed by Dr. Mitchell's observations.

## DIGESTION.

Bouchardât and Sandras have found that cane sugar, in order to be destroyed in the blood, must previously be changed into inverted sugar, or converted into lactic acid in the alimentary canal, the ultimate product of this destruction being water and carbonic acid. These experimentalists have also found that raw starch, though imperfectly assimilated by man, is well digested in the stomachs of the ruminantia.\*

In the admirable essays of the Messrs. Goodsir, of Edinburgh, we meet with a probable solution of that most difficult question of lacteal absorption. It is effected, they suggest, by means of nucleated cells, those universal agents in the organic kingdom. As the chyme passes along the intestines, the epithelia are thrown off which cover the villi, so that the chyme comes fully in contact with their exposed surface. Scattered among the terminal loops of the lacteals in each villus, are numerous granular particles, which are probably the germs or nuclei of the absorbing vesicles, and these become fully developed during the process of chyme absorption. These vesicles (probably by endosmose) draw into their interior certain materials from the chyme surrounding the villi, and they subsequently burst and discharge their contents into the lacteals.†

\* *Lancet*, March, 1845, p. 231.

† *Anatomical and Pathological Observations*, 1845, p. 4.

An opposite view of the process of lacteal absorption has been taken by a diligent and able observer, Mr. Samuel Fenwick, of North Shields. From the numerous and carefully conducted experiments which he has made, he concludes that the lacteals obtain their contents solely through the medium of the blood-vessels. His papers on this subject deserve attentive perusal.\*

*Liver and Bile.*—Mr. Paget fully concurs with Müller in upholding Kiernan's views of the structure of the liver; but he states that Müller is in error when he describes the partitions of that organ to be formed of fibro-cellular structure.

A new test, equally efficient for discovering the presence of bile as well as that of sugar in animal fluids, has been proposed by Pettenkofer. This author, a pupil of the Giessen laboratory, founds his process on the fact, that when ox gall had been treated with sugar, and concentrated sulphuric acid was then added until the precipitate of choleic acid had begun to re-dissolve, the mixture became considerably heated, and the liquor assumed a deep violet tint, similar to that of permanganate of potass. This process may, of course, be inverted for the detection of sugar.

## UTERUS AND ITS FUNCTIONS.

*Puberty.*—Mr. Roberton,† of Manchester, has obtained additional data respecting the age at which puberty commences in warm and in cold countries. It has been generally believed that menstruation occurs earlier in tropical and warm climates than in the regions nearer to the poles; but Mr. Roberton shows, from the answers obtained from the Moravian missionaries on the coast of Labrador, that the Esquimaux females there menstruate as early as the women in this country, the average age of the appearance of the menses in Labrador being fifteen years and a half. The catamenia cease among the Esquimaux females at a somewhat earlier period than with the women in this country; but may not this be a consequence of the hardships of their mode of life? The reports upon this subject which Mr. Roberton has received from Greece, also prove that puberty in that favoured clime does not commence earlier than on the inhospitable coast of Labrador.

\* *Lancet*, January, 1845.

† *Edinburgh Medical and Surgical Journal*, January, 1845, p. 57.

*Menstruation.*—Much has been recently published regarding the theory of menstruation. The observations of Mr. Girdwood,\* of Raciborski,† and above all of Bischoff‡, tend to prove that menstruation in the human female is analogous to the rut, or coming into heat, of animals, and that at each menstrual period a species of attempt at elaboration of an embryo, if we may use such an expression, takes place in the female ovaries. This explains the now pretty well established fact, that conception most frequently takes place immediately before or after the menstrual period. Mr. Girdwood's observations upon animals confirm also the opinion of Cuvier and others, that a secretion analogous to that of the menses occurs in the higher mammalia; nor is its sanguineous character solely confined to the human female. In woman and in the females of all animals in which the periodic discharge is apparent, this indicates always the maturation of an ovum, which is on the point of being eliminated from the ovaries, and the capability for impregnation is probably at its acme during the menstrual period or towards its close. Bischoff also states, that the bursting of the Graafian follicles, and the consequent discharge of ova, may take place perfectly independent of contact with the spermatozoa, or of sexual orgasm. The memoir of M. Raciborski,§ on “*Corpora Lutea*,” though too long for analysis here, should be carefully studied by all who wish for information on this interesting subject; and we can recommend for perusal the elaborate memoir on the “*Structure of the Uterus*,” by M. Jobert de Lamballe.||

*Embryology.*—Dr. Allen Thompson has continued his researches in embryology, and has lately published some very curious observations on the early condition and probable origin of double monsters. All true double malformations he believes to be accompanied with a double condition of the cerebro-spinal axis, and that consequently their duplex state is of original formation, or dates at least from the earliest stages of development.

Malformations by redundancy of parts should be divided into two classes, the one including redundancy of unimportant parts,

\* *Lancet*, December 14, 1844.

† *Bulletin de l'Académie*, November, 1844.

‡ See a translation of his Memoir, by Mr. H. Smith; *London Medical Gazette*.

§ *Edinburgh Medical and Surgical Journal*, April, 1845, p. 514.

|| Dr. Ranking's *Half-Yearly Abstract of the Medical Sciences*, July, 1845, p. 278.

the other comprehending the double monsters, strictly so called. In the latter cases the ovum, according to Professor Thomson, is malformed from the very beginning. The writer also concludes, from a case observed at a very early period of development, that the rudimentary form, or first trace of an embryo in a double monster, is a simple groove, bounded on either side by dorsal plates, as in a single foetus. The two primitive grooves thus lying side by side, become agglutinated together by the parts in immediate contact, and in these last there ensues a certain amount of arrest of development from want of room. In such cases a single visceral cavity may easily come to co-exist with a double cerebro-spinal axis.

#### MISCELLANEOUS SUBJECTS CONNECTED WITH PHYSIOLOGY.

*Structure and Formation of Tissues.*—Our limits do not permit us to do more than generally to refer to the admirable essays of the Messrs. Goodsir upon this and other important subjects. There is not a page of their work which is not replete with valuable information.

*Bone.*—M. Flourens maintains that bone is formed in the periosteum; that it increases in size by the addition of external layers; and that the medullary canal augments its dimensions by absorption of the internal layers of the bone.\*

Dr. Watson comes to the following conclusions regarding the formation of bone by the periosteum:—

1. That the theories alleging that new bone is formed only by the living parts of the old bone, in cases of necrosis and fracture, are incorrect.
2. That the periosteum has evidently the power to produce new bone of itself, without the aid of the old bone.
3. That the formation of new bone by the periosteum consists, at first, in the deposition of osseous matter in the form of a fine microscopic network, and therefore that the Haversian canals are only a secondary, and not a primary formation of the osseous tissue.†

\* *Archives Générales de Médecine*, October, 1844, p. 254.

† *Edinburgh Medical and Surgical Journal*, April, 1845, p. 307.

Dr. Stark has found oil in all the bones of animals; even the dense shaft of the cylindrieal bones of the extremities contains a notable proportion, of from 4 to 25 per eent., independent of that known to exist in the marrow. He states that it is a fallacy to suppose that the bones eontain a larger proportion of animal matter the higher we advance in the scale of organization. The following are the additional results of Dr. Stark's researches on this subjeet:—

1. The animalized base composes very nearly an exact third of the weight of the dry clean bone.
2. The proportion of earthy matter in the bones of the wild mammalia seems to be a fraction higher than in the domesticated animals. The bones of animals artificially fed, and deprived of their usual exercise, present a rather smaller amount of earthy salts.
3. Age does not seem to increase the amount of earthy matter in the bones.
4. The hardness of bones does not depend on the amount of earthy matter contained in them, nor do the more flexible bones appear to be deficient in earthy salts.
5. The transpareney of bones does not depend upon a defieieney of earthy matter.\*

*Serous Membranes.*—Dr. Robert Willis regards the serous membranes as internal substitutes for the external common integuments, and is of opinion that the fluid which perpetually bedews the surface of these membranes is analogous, as to its intent, with the perspiration on the skin. Dr. Willis believes that the exudation on serous membranes is carried off and distributed by the absorbents, and he thus seems to regard nutrition as a process of universal secretion.

The eommon theory, which ascribes the serious constitutional sympathies in cases of inflammation of serous membranes, to interference with the motions of the organs which they cover or line, is rejeeted by the writer, who believes it is rather that the nutrition and vitality of the organs eoneerned are compromised, and that the conditions essential to the aecess of the nutritive fluid, and to the removal of effete matters, are interfered with.†

\* *Edinburgh Medical and Surgical Journal*, April, 1845, p. 308.

† *London Medical Gazette*, March, 1845, p. 767.

## PATHOLOGY AND THERAPEUTICS.

We now come to the more practical division of our subject, the consideration of the diseases affecting the human body, and of the treatment best adapted to their cure. We shall follow here nearly the same order as we have already pursued in the physiological portion of this Address, and shall therefore first call attention to what has recently been written on the diseases of the brain and of the nervous system.

### DISEASES OF THE NERVOUS SYSTEM.

Through the unceasing exertions of British and continental physicians we have been furnished with some additional data respecting this obscure class of diseases, but much of what has been advanced rests upon conjecture or analogy; nor can we hope for more positive observations and doctrines till our knowledge of the physiology of the nervous system is more extended than at present.

*Softening of the Brain.*—The pathology of cerebral softening and haemorrhage has been carefully studied by that veteran observer, M. Rochoux, of Paris.\* Softening of the brain, according to this author, occurs under three different forms, which must be carefully distinguished one from the other. These three conditions are:—

1. Softening preparatory to haemorrhage. (Ramollissement hæmorrhagique.)
2. Infiltration of blood.
3. Inflammatory softening.

With respect to the first of these three forms, M. Rochoux denies, in very pointed terms, the assertion that haemorrhage ever occurs in the brain without previous softening.

Under the second condition, (infiltration of blood,) our author describes minutely the red points, termed capillary apoplexy by Cruveilhier, and so accurately described by Rokitansky and Koletscka, though the French physician seems totally ignorant of the researches

\* *Archives Générales de Médecine*, November, 1844, p. 268; and December, 1844, p. 401.

of his German brethren. These appearances cannot, he thinks, occur in a previously healthy brain, as in that condition this organ is capable of resisting the most violent congestion, such as is known to occur, for instance, during the epileptic paroxysm. M. Rochoux believes these points to be formed by infiltration of blood through the coats, and not by actual rupture of the vessels, and also that they admit readily of being removed by appropriate treatment; and that after death from other causes, no cavity can in such cases be discovered, but only a sort of spongy areolar tissue, traversed by large and well-defined cerebral fibres.

To distinguish after death the third, or inflammatory form of softening of the brain from that preparatory to haemorrhage, M. Rochoux observes, that in the latter case no effect is produced upon the brain by a stream of water directed upon its substance, whereas the whole is immediately broken up by this process when softening is the result of inflammation.

Finally, we have the important remark that it is not essential to the existence of inflammation in the cerebral substance that the characteristic redness should be present, as there is little or no cellular tissue in the brain; and on this last depends the amount of colouring in inflammation.

The observations of Dr. J. H. Bennett\* are in many respects opposed to those of M. Rochoux. Dr. Bennett does not believe that softening necessarily precedes haemorrhage in the brain. His observations on contraction of the limbs, as a symptom of inflammatory softening of the brain, are extremely interesting.

*Paralysis.*—Mr. Steclé† has added another of those curious cases, first detailed and examined by Dr. Abercrombie, where the intellect remained apparently unimpaired after repeated slight apoplectic seizures, save that the individual had lost all power of forming words correctly.

Another instance of those perplexing cases for physiologists, where the lesion in the brain was found to be on the same side as that on which paralysis occurred, has been related by Signor Freschi.‡ It is probable that, in this case, the enormous extent

\* *Edinburgh Medical and Surgical Journal*, July, 1844, p. 351.

† *Dublin Journal of Medical Science*, January, 1845, p. 355.

‡ *Lancet*, 1844, p. 304; from *Bulletino delle Scienze Medicale*.

of the lesion discovered on dissection may account for the anomaly above referred to.

In the *Practical Observations in Medicine*, published this year by that indefatigable observer Dr. Marshall Hall, we find some most valuable hints regarding the prevention and treatment of apoplexy and paralysis. Dr. Hall justly insists on the dangerous tendency of that once commonly received opinion, that all cases of this nature depend on a plethoric state of the system, and confirms the doctrine of the most enlightened observers of the present day, that apoplectic symptoms may arise equally from anaemia, from dyspepsia, from diseases of the urinary organs, or from the gouty diathesis. Dr. Hall recommends, in doubtful cases, that blood should be taken by a large orifice, from the patient when in the erect position; for should plethora have occasioned the dangerous symptoms, the loss of blood would be well borne to a great extent without syncope, and with decided benefit to the patient; while, on the other hand, if the vertigo and other signs have arisen from dyspepsia, gout, or an anaemic condition of the system, fainting would rapidly supervene, and would assist in leading to a correct diagnosis of the nature of the case.

The faculty of speech has been supposed by Dr. Cowan, of Reading, to reside in the anterior lobes of the brain, and this is corroborated by a case which occurred in the practice of Dr. Powell,\* of Tonbridge, where the lesion after death was found to be located in these parts. Mr. Steele's case, above referred to, was perhaps also of this description.

Some curious observations, though more immediately connected with physiology than practical medicine, have been made by Dr. Bernard,† of Paris, on four cases of paralysis of the portio dura of the seventh pair, in all of which the power of tasting with the tongue was remarkably defective.

The hot-air bath has been found useful in a case of sudden and complete paralysis of the fore-arm, related by Dr. Kennion,‡ of Harrowgate. At the end of three weeks the patient recovered the complete use of the limb, without having taken any medicine internally. We think that something may be allowed to the *vis medicatrix naturæ* in this case.

\* *Provincial Medical and Surgical Journal*, May 1845, p. 541.

† *Archives Générales de Médecine*, December, 1844, p. 480.

‡ *Provincial Medical and Surgical Journal*, June, 1844, p. 142.

*Hydrocephalus Acutus.*—The value of hydriodate of potass in hydrocephalus acutus has been strongly urged by Amelung, of Darmstadt,\* in corroboration of the favourable report given of its powers by Röser, of Bartenstein. Dr. Amelung has found this preparation particularly valuable in that form of the disease which occurs after scarlatina, but he thinks the doses proposed by Röser (eight grains to a child of four years old) too large, or at any rate unnecessary; he believes an equal effect is produced by four grains in divided doses, daily, to a child of that age. But Röser gave three drachms of the hydriodate of potass to a child of three years and a half old, in twenty-four hours, and it recovered!† In a fatal case, where he believes that the iodine was too long delayed, the fluid in the ventricles of the brain gave a very distinct violet reaction with starch, after it had been previously treated with nitric acid, whence Röser concludes that the remedy is conveyed through the system in the form of hydriodate, and not of pure iodine.

The scalp issue of Dr. Wallis, of Bristol, has been successfully employed by Dr. Oke,‡ of Southampton, in this disease, but is deemed unnecessarily severe by Dr. Blackmore, in his excellent essays on nervous diseases.§ Dr. Blackmore prefers a deep incision in the scalp, and allowing the wound to bleed freely; though it is justly observed by Dr. Oke, that patients requiring these remedies are seldom in a condition to feel acutely from any operation. Too many distinct and separate maladies are still, we fear, classed under the name of hydrocephalus acutus.

*Meningitis.*—An acute form of this disease appears to have prevailed epidemically at Gibraltar during the early months of last year. Dr. Gillkrest states that it proved fatal in 45 cases out of 450. Several cases of meningitis are recorded in various periodicals: that detailed by Mr. Adams, in the *Dublin Journal of Medical Science*, is interesting, from the analogy observed between the symptoms and the appearances after death; and still more, from its furnishing another glaring instance of maltreatment by the so

\* *Journal der Praktischen Heilkunde*, 1844.

† *Medizinische Annalen*, p. 481, 1843.

‡ *Provincial Medical and Surgical Journal*, September, 1844.

§ *London Medical Gazette*, March, 1845, p. 725.

called homœopathic physicians. We can refer with pleasure to an interesting essay on inflammatory diseases of the brain and of its membranes, by Mr. Vines, of Reading.\*

Of Dr. Cowan's† excellent observations on encephaloid tumours of the brain, it is not possible to give a complete analysis. Dr. Cowan is disposed to think, from what he observed in the two instances he has recorded, that where the local and general symptoms justify the diagnosis of an organic affection of the brain, and when such symptoms are accompanied with remittent or intermittent pains of a neuralgic character, with gradual emaciation and a cachectic condition of the system, we may, with every probability, infer the existence of malignant disease. In one of the cases related by Dr. Cowan, a constant whizzing pulsatory noise was heard in the right ear, and on dissection after death an encephaloid mass was found lying on the temporal bone of that side.

*Tetanus.*—Opportunities, unfortunately but too frequent, have this year been afforded of testing the virtues of various boasted new remedies in this terrible disorder, and of proving that their efficacy, if any, was too slight and transient to entitle them to the fame of specifics. The Indian hemp has failed in the hands of Mr. Stafford,‡ Mr. Potter,§ and Dr. Babington;|| or, at the best, it afforded only temporary relief, though the utmost care was taken to obtain the drug in a state of purity. We do not, however, wish to infer that the extract of Indian hemp is totally valueless in this disorder. The cases recorded by Mr. Donovan and Professor Miller¶ are strongly in favour of its efficacy in allaying nervous irritation.

From the well-known sedative effects of intoxicating fluids on the nervous system, Mr. Stapleton,\*\* of Trowbridge, was induced to employ alcohol in tetanus. Although he did not succeed in saving the patient's life, yet the influence of the alcohol in

\* *Provincial Medical and Surgical Journal*, January 1845, p. 13.

† *Ibid*, April 16, 1845, p. 237.

‡ *London Medical Gazette*, April 29, 1845.

§ *Lancet*, January 11, 1845, p. 36.

|| *Ibid*, December 14, 1844, p. 352.

¶ *London and Edinburgh Journal of Medical Science*, January, 1845, p. 23.

\*\* *Lancet*, March 22, 1845, p. 317.

diminishing the violent paroxysms was so obvious that the remedy, in so generally hopeless a disorder, is certainly worthy of trial. Mr. Stapleton attributed its failure to the long presence of the disease before the remedy was determined to be employed. A successful case, strongly corroborative of Mr. Stapleton's views, has recently been brought before the Medico-Chirurgical Society by Dr. Wilson.\* In this well-marked instance opium had been tried in vain, and the patient was apparently saved from impending death by swallowing enormous quantities of brandy, not less than two gallons of that spirit, with a large allowance of wine, having been taken in the course of eight days.

For some excellent observations on the pathology and treatment of tetanus, I may refer to a paper by Dr. Inglis;† and this essay is rendered of increased value by the excellent rules that are there laid down for distinguishing the true from the spurious extract of Indian hemp.

*Chorea.*—Very little light has been thrown of late years upon this obscure nervous disease and its treatment. We have ourselves succeeded in curing some cases by arsenic, and others by zinc, where arsenic could not be borne; but in some instances we failed altogether, by any remedies, to relieve the symptoms, though of these a few afterwards recovered by change of air, and without medical treatment.

One case, induced by fright, in a girl of nine years old, is recorded in the *Lancet* to have been treated by the potassio-tartrate of antimony, after every other remedy had failed, and the patient eventually recovered. The case of intermittent chorea recorded by Nue, of Vordingborg, in the first number of the *Bibliothek for Læger*, for 1844,‡ is obviously connected with hysteria. The girl was nearly twelve years of age. The paroxysms occurred every hour, exactly as they were announced by the clock, (which by the way hung at the bed head,) and continued daily from six in the morning till seven in the evening. Our worthy Danish brother arrested the return of the paroxysm, by a few severe reprimands addressed to the patient.

\* April 22, 1845; *vide Medical Times*, May, 1845, p. 84.

† *Provincial Medical and Surgical Journal*, February and March, 1845.

‡ *Bibliothek for Læger*, 1844, p. 193.

*Hydrophobia.*—In the present state of our knowledge the successful treatment of this fearful disorder seems to be even more hopeless than that of tetanus. Dr. Hooper, of Buntingford\*, records one instance, where he subdued evident symptoms of hydrophobia by calomel and opium; but in Mr. Jackson's† case, an ounce and a half of calomel was given in twenty-four hours, without arresting the disorder. Dr. Hooper thinks that salivation should be as rapidly as possible induced; but herein lies the main obstacle, for it is extremely difficult to obtain in such cases the specific action of mercury, though if produced, a favourable result may be expected. The extreme period during which the poison of hydrophobia may remain dormant in the system has not yet been determined. A well authenticated case of the outbreak of this malady, two years after the bite of a rabid dog, is recorded in a German work of high authority;‡ and another instance, embracing an exactly similar period of time, occurred in our own practice, in November, 1840.

*Neuralgia.*—Few will deny at the present day that a great proportion of neuralgic diseases are connected with derangement of the digestive organs; and a theory as to the direct cause of these pains has been founded partly on this acknowledged fact, and promulgated by several parties at the same time. The explanation proposed is, that those periodic congestions, so well known to occur in gouty and dyspeptic habits, are in neuralgia thrown upon the blood-vessels accompanying the nerves; and these latter being confined in unyielding bony canals, especially at their exit from the skull, are necessarily compressed by the overloaded vessels, and pain of a violent character is induced.

Dr. Allnatt has called our attention to the great variety of neuralgic complaints arising from dyspepsia, and he has added some curious instances of hepatalgia, in which perfect alternations of pains between the face and the hepatic region were observed.

With Mr. J. M'Veagh, Dr. Allnatt believes that all, or the greater part, at least, of true neuralgic affections, will yield to gentle purgatives, combined with colchicum, ipecacuanha, and

\* *Lancet*, May, 1845, p. 158.

† *Provincial Medical and Surgical Journal*, November, 1844, p. 496.

‡ *Abhandl. der K. K. Gesellsch. der Aerzte zu Wien*, vol. i.

hyoscyamus; and if the constitution have suffered from protracted unmitigated pain, alkaline vegetable tonics will effect a cure, which we might in vain expect from the more potent mineral preparations.

Inoculation with the salts of morphia has been tried with good success by Dr. Jacques, of Antwerp, and by Mr. Rynd, in this country.\* With a lancet dipped in a pretty strong solution of sulphate of morphia, Dr. Jacques makes a number of punctures over the affected part, and repeats the operation for several successive days. M. Lafargue has obtained favourable results from the employment of veratrine in a similar manner.

Dr. D. MacLagan† and Professor Simpson have introduced to the notice of the profession a new preparation—the sulphate of bebeerine—an alkaloid, from the bebeeru or green-heart tree of British Guiana. They have tried this new remedy with success in several cases where quinine and iron were contra-indicated by the constitution or condition of the patient. In pregnant females with a tendency to miscarriage, and at the same time suffering from neuralgia, Professor Simpson found the alkaloid of great value, as quinine or iron could not be exhibited in these instances without imminent risk of causing abortion.

*Epilepsy.*—According to Dr. Corrigan,‡ of Dublin, digitalis has long been a popular remedy in Ireland, for the cure of epilepsy, though the doses in which it has been given are certainly too powerful. Not less than four ounces of the strongest infusion of the leaves are taken by the lower classes every third day, with fifteen grains of the root of the polypodium. Under the influence of such exaggerated doses, much irritation of the gastric mucous membrane is necessarily occasioned, with fearful sinking of the vital powers. Dr. Corrigan gives an ounce and a half of the infusion of digitalis, of the *Dublin Pharmacopœia*, every night at bed time, and increases the dose gradually up to three ounces, with decided effect on the disease. We should not ourselves recommend this plan of treatment, where the paroxysms arise, as they so frequently do, from irritation of the digestive organs.

\* From *Provincial Medical and Surgical Journal*, July 3, 1844.

† *Edinburgh Medical and Surgical Journal*, April, 1845.

‡ *Dublin Hospital Gazette*, May, 1845.

## DISEASES OF THE RESPIRATORY ORGANS.

*Asthma*.—The doubtful pathology of asthma has received little or no elucidation during the past year. Professor Henle,\* of Zurich, in an excellent essay upon certain disorders of the bronchi, positively asserts the contractility of the walls of these air-passages, and believes that asthma may be caused either by cramp or by paralysis of their fibres.

The caroub of Judea, a parasitic production formed on the leaves of the Pistacia Terebinthus, has been tried by M. Martin Solon † in this disease. He does not consider it to possess any powers superior to those of the resinous and balsamic preparations in general use.

Mr. J. B. Harrison ‡ has proposed the inhalation of the fumes of the nitrate of potash in this complaint. The remedy is of American origin, and at all events, from its great facility of application, is worthy of a trial.

*Croup*.—Dr. Ellis, of New York, recommends the croton oil ointment or liniment in croup and aphonia; and Valleix,§ of Paris, acknowledges, with Rokitansky, the identity of croup and diphtheritis, regarding them, with the Professor of Vienna, as differing only in situation.

*Œdema Glottidis*.—The name of laryngite œdemateuse, applied to this disorder by Cruveilhier and others, is justly, we think, objected to by Dr. Lasiauve,|| of the Bieêtre, who denies that this malady arises always from inflammation. Dr. Lasiauve believes that œdema glottidis may and does frequently occur passively, in consequence of obstructed circulation in various parts of the body.

*Pneumonia*.—The question of the exact locality of this disease in the lungs remains at present undecided. Dr. Addison, Professor Rokitansky, and others, believe its seat to be within the air-cells;

\* *Zeitschrift für rationelle Medizin*, 1844, p. 249.

† *Bulletin de l'Académie de Médecine*, September, 1844.

‡ *Lancet*, April 5, 1845, p. 383.

§ *British and Foreign Medical Review*, October, 1844, p. 300.

|| *Annales de la Chirurgie Française*, &c., November, 1844.

while M. Prus,\* with an equal weight of authorities, declares it to be essentially an inflammation of the intervesicular cellular tissue, and that it does not necessarily involve any lesion of the bronchial tubes or air-cells. A most elaborate article on the "Pathology of Pneumonia," has been published in a German periodical, by Dr. Himmelstern, of Moscow.† In many respects this author's investigations coincide with those of Dr. Addison. He denies that grey hepatization is a *progressive* stage in pneumonia; he considers it merely as the process of resolution or softening, and that it is not a purulent infiltration of the pulmonary parenchyma, but is merely caused by the lymph, which has been effused on the interior surfaces of the air-cells, undergoing the process of softening preparatory to its being expectorated from the bronchi. This softening, however, may occasionally extend itself beyond the walls of the cells, and embrace the neighbouring parenchyma, which then becomes the seat of a true pulmonary abscess.

An excellent report upon forty-four cases of pneumonia, observed in the Royal Hospital of Christiania, has appeared in the *Norwegian Medical Journal*.‡ The proportion of deaths in these cases was one in eleven. Pneumonia of the right lung occurred in twenty individuals; of the left, in twelve only; while twelve other cases presented unequivocal signs of double pneumonia; still in these last the malady appeared to have commenced on the right side of the body. In one case no expectoration whatever occurred throughout the whole progress of this disease.

Much has been lately written upon the value in the diagnosis of pneumonia of the minute crepitating sound, so much insisted upon by Laennec. It seems to be a prevailing opinion that a very similar sound may be produced by small particles of lymph upon the surface of the pleura; though as these asperities (if we may so term them) on that serous membrane seldom occur without effusion of fluid into the cavity of the pleura, it is possible, as is suggested by Dr. Macdonell, of Belfast, that the crepitation alluded to is merely the result of air re-entering the previously compressed vesicles of the lungs. But if, on the other hand, it be true that,

\* *Revue Médicale*, April, 1845.

† *Haeser's Archiv.*, 1843, p. 576.

‡ *Norsk Magazin for Lægevidenskaben*, 1843.

according to Dr. Durrant,\* of Ipswich, perfect pneumonic crepitation is never heard but on inspiration, then the analogy between the two sounds will no longer hold good; for it is obvious that if the sound be produced by the rubbing of the pleura, it must be audible also on expiration.

In the treatment of pneumonia we have advanced but little. Dr. Gouzée,† of Antwerp, recommended small doses of morphia in the delirious state, which often continues after the subsidence of the inflammatory symptoms; but his doses are extremely small, being only  $\frac{1}{24}$ th of a grain of morphia every six hours.

Dr. Upshur, of New York,‡ has given the iodide of potassium in pneumonia with great success, especially in the so-called third stage of that disease. The advantages of this remedy have been chiefly apparent in those cases eminently characterised by depression of the vital powers of the system.

From the experiments of Gluge and Thiernesse,§ it appears that animals fed upon daily increasing doses of oil die, after some time, with all the symptoms of violent pneumonia.

*Gangrene of the Lung.*—One of those rare instances of recovery, after every symptom of gangrene of the lung, has been recorded by Dr. Tinniswood, of Carlisle.|| Several portions of pulmonary parenchyma were observed in the horribly foetid matter expectorated. In two other cases of gangrene of the lungs, related by Dr. Watson, of Liverpool, the bronchial tubes were found to have resisted the process of destruction longer than the cellular tissue or the parenchyma of the lungs, as they lay loose along with the blood-vessels in the gangrenous cavity.

*Pleurisy.*—In the year 1830, Dr. Wendelstadt, of Hersfeld,¶ communicated to *Hufeland's Journal* the history of his own sufferings from pleurisy, accompanied by empyema, for which the operation of paracentesis of the thorax had been performed upon him in 1817. From this last period, and up to the year 1843, the

\* *Provincial Medical and Surgical Journal*, January, 1845, p. 65.

† *Archives de la Médecine Belge*, March, 1845, p. 123.

‡ *Lancet*, February, 1845.

§ *Gazette Médicale de Paris*.

|| *London and Edinburgh Journal of Medical Science*, July, 1844, p. 548.

¶ *Kleinert's Repertorium*, May, 1844, p. 73.

fistulous opening in the pleura continued to discharge large quantities of pus, and there was evidently also a communication between the bronchi and the sac of the pleura, as the patient could by a sudden effort extinguish a candle placed within six inches of the fistulous opening. Dr. Wendelstadt, though much debilitated, was able to continue his professional duties till March, 1843, after which time he was confined to the house, and died in August of that year, the thirty-third from the commencement of the disease, and the twenty-seventh from the date of the operation. The left lung and pleura were quite healthy, but of the right lung only a trace remained, consisting of a mass six inches long and one inch and a half broad and thick, and scarcely permeable to air, but through whose substance, nevertheless, there existed a communication between the bronchi and the pleural cavity. This last, as might be expected, was very thickly lined with false membrane.

Instances where the fluid of empyema makes its way into, and is expectorated from, the bronchi, are by no means very rare: but in one case, recorded by Dr. Macdonell,\* it was remarked that the breath, on the patient coughing or making a violent expiration, was intensely foetid, while it was not so on ordinary respiration. Dr. Macdonell accounts for this phenomenon, by supposing that the more distant bronchial ramifications were compressed by the effusion, and that the air contained in them became putrid, and was only evacuated from the chest upon the more violent exertion of coughing. In this case, too, a loud single *bruit de soufflet* was heard in the descending aorta, which in all probability was occasioned by the pressure of the distended pleura upon that vessel, with which it is there immediately in contact. As, however, this sound was not audible in eight other cases of empyema of the left pleura, we are justified in concluding, with Dr. Macdonell, that it can only occur when the pleuritic effusion is tightly bound down by false membranes.

M. Troussseau† has lately entended the operation of paracentesis to the cure of acute as well as of chronic empyema. The three patients on whom the experiment was tried all slowly recovered, though they are stated to have been almost moribund when the operation was performed.

\* *Dublin Journal of Medical Science*, January, 1845, p. 434.

† *Archives Générales de Médecine*, September, 1844, p. 103.

*Hooping-Cough.*—The paroxysms of hooping-cough are believed by Mr. Streeter\* to be occasioned by the irritating nature of the saline secretion poured out in that disease. This theory, however, is not new; it was originally promulgated by M. Blaud, in 1831, who stated also that the above named secretion contained a very large proportion of hydro-chlorate of soda.

*Phthisis.*—The past year has presented the usual number, or even more, of essays, observations, and separate cases, relating to this disease. We must, however, acknowledge that no strikingly novel views of the pathology of tubercle, nor any advances in the treatment of its various stages, have been laid before the public. Our only hope is, that the true nature of the disease, and its appropriate treatment, may finally be discovered by steady perseverance in those researches by which so many have of late distinguished themselves, viz., the closest microscopical and chemical investigations, guided by a sound and cautious pathology. Some of these we shall now proceed to notice. One of the first questions that presents itself regards the nature of the grey granulations observed in tubercular lungs. From the time of Laennec and Bayle they have formed a subject of controversy. This year we would say that the balance inclines more towards the opinion of Andral and others, that they are not of a tubercular nature, but should be considered as the result of chronic pneumonia. If such really be the case, as is asserted positively by Lombard, of Geneva,† these may soften and form small cavities, and if ever closely aggregated may form large excavations without passing through the stages of tuberculization. Rampold, of Esslingen, is decidedly of this opinion.‡

The aspect of tuberculous matter, at the very first appearance in the system, has been described by M. Rochoux§ to be that of a filamentous tissue, singularly interlaced, and this gradually assuming a pale orange colour, becomes miliary tubercle.

The chemical constituents of tubercle have been as variously stated as their origin and growth. M. Boudet|| reiterates his

\* *London Medical Gazette*, November, 1844, p. 195.

† *From Northern Journal of Medicine*, February, 1845, p. 254.

‡ *Kleinert's Repertorium*, March, 1844, p. 65.

§ *Académie des Sciences*, December 30, 1844.

|| *Archives Générales de Médecine*, October, 1844, p. 247.

assertion, that the calcareous concretions found in phthisical individuals, especially in the lungs, are composed chiefly of soluble salts, namely, 70 per cent. of the chloride of sodium, and of phosphate and sulphate of soda, with but a small amount of the phosphate and carbonate of lime. M. Boudet's opinions are objected to by Dr. S. Wright, of Birmingham,\* who has invariably found these calcareous bodies to consist of the phosphate of lime alone, or in combination with the carbonate. Boudet also states that he has found casein in pulmonary tubercles, and that he also constantly observed them to contain a considerable proportion of cholesterine,—not less than one-twentieth part of the dry tuberculous matter.

The contradictory opinions I have here enumerated show that much must be done ere this question can be finally settled.

Besides the essays upon tubercular disease by Dr. Evans, of Dublin,† and by Dr. S. Wright, of Birmingham, we have this year two very valuable papers on this subject from the pens of Dr. Addison, of London,‡ and Dr. J. H. Bennett, of Edinburgh.§ Dr. Addison chiefly directs our attention to the important part played by inflammation in all cases of so-called tubercular disease. He denies that tubercular infiltration is entitled to that name, but states it to be always the result, not of tubercular deposit, but of a slow and insidious form of pneumonia. In proof of this assertion, he refers to many cases where, though the whole lung was infiltrated with the so-called tuberculous matter, no single tubercle could anywhere be detected. Many, we think, will be disposed to believe with Dr. Addison, that the majority, if not all, of the so-called galloping consumptions, where the patient is often carried off in a few weeks, are in reality instances of a peculiarly insidious form of sub-acute pneumonia. Dr. Addison describes phthisis under three separate heads:—

1. Pneumonic phthisis, to which we have already referred.
2. Tuberculo-pneumonic phthisis, where the patients die of pneumonia, though tubercles are present in the lungs.
3. Tubercular phthisis, properly so called.

\* *Medical Times*, January and February, 1845.

† *Lectures on Pulmonary Phthisis*; Dublin, 1845.

‡ *Guy's Hospital Reports*, April, 1845.

§ *Edinburgh Medical and Surgical Journal*, April, 1845, p. 406.

Dr. Addison's essay should be carefully studied.

Although Dr. Bennett professes merely to treat of the curability of phthisis in his essay, still we find that he has carefully selected the most approved doctrines regarding the origin and nature of tubercles, and has appended to these his own valuable observations. Dr. Bennett considers tubercles to be in a great measure identical with, or analogous to, lymph. Both are the products of inflammation, granular and perfect cells being formed in the one case, and granular but imperfect cells in tubercle. Gulliver, Vogel, and others, have asserted tubercle to be composed of imperfectly nucleated cells; but this is denied by Dr. Bennett, Lebert, and a host of talented observers. Dr. Bennett does not admit the absolute origin of tubercle from inflammation, without qualifying this opinion by the statement, that the essential difference between tubercle and the products of normal inflammation, consists in a difference, chemical and vital, of the blood-plasma of which they are composed, and which in tubercle is probably some form of protein less capable of passing into organization as fibrin. The principal object of Dr. Bennett's paper is, as we have said before, to prove the greater frequency of the cure of phthisis than is generally supposed. For some years this has been a favourite doctrine of several eminent pathologists in France; and we ourselves entirely concur with Dr. Bennett, as to the much greater frequency of calcareous concretions, puckerings of the lung, and the other signs which he enumerates, than has been generally admitted by English writers. The question is, however, how far these *post-mortem* appearances are really indications of a cure. Out of 308 cases examined by Boudet, Rogée, and himself, Dr. Bennett states that more than one half presented the above conditions in the lungs. The great difficulty in the cure of phthisis will, we think, ever continue to be the constitutional tendency to further deposit. In reference to this part of our subject we may notice the much disputed question of the influence of a malarious climate in arresting the course of phthisis. M. de Crozant\* has confirmed, in this respect, the observation of Boudet; but it must be acknowledged that a plan of treatment founded on this doctrine, will only substitute one evil influence for another, which, though less fatal, will equally embitter the patient's existence. Opium

\* *Journal de Médecine*, May, 1844.

has been extolled by Forget, of Strasbourg,\* as almost a specific in the cure of consumption. Naphtha, as a remedial agent, has never obtained the confidence of the profession; and Mr. Rose, of Norwich, bears further testimony against its value in this disease.

Our highest living authority in phthisis, M. Louis, has been lately engaged in a series of interesting trials, of a plan proposed by a French physician for the cure of phthisis.† M. Türck had stated that consumption might be effectually cured by confining phthisical patients to a room, kept uniformly at a high temperature, and of which the atmosphere was strongly impregnated with ammoniaeal vapours. M. Louis tried this plan on seven individuals, five males and two females, at the Hôpital Beaujon. The results were very unsatisfactory; no real amelioration was produced; two, indeed, seem to have died from capillary bronchitis induced by the treatment, which was so irksome and annoying to the patients that only two resolutely submitted to it for a long period of time. Of other remedies, cod-liver oil appears now to be going out of favour, in spite of the high encomiums passed upon this remedy by Pereyra, of Bordeaux; and of the operations of opening the chest from without, in tubercular excavations, it is scarcely necessary here to speak, as it has only once been attempted in this country, and that under such auspices as would not induce a recurrence to such a questionable mode of cure.

## DISEASES OF THE CIRCULATING SYSTEM.

*Functional Disorder.*—Professor Christison,‡ of Edinburgh, has called the attention of medical men to the frequency of cases of functional heart complaints, simulating hypertrophy of that organ. The disorder he alludes to is characterised by violent pulsations of the heart, which are most felt near the point of the sternum and over the left costal cartilages. The palpitations usually attain their maximum when the patient retires to rest at night; but the affection may be distinguished from organic disease by being unaccompanied by dyspnœa, and by the absence of all abnormal signs on auscultation and percussion. Exercise, if not too violent, tends

\* *Bulletin Générale de Thérapeutique*, December, 1844.

† *Archives Générales de Médecine*, December, 1844.

‡ *London and Edinburgh Journal of Medical Science*, February, 1845, p. 81.

rather to mitigate than to increase the malady. Professor Christison believes these symptoms to be connected with a small heart and a highly excitable temperament, and that dyspepsia cannot be regarded as its sole cause, though he acknowledges that the functions of nutrition and assimilation are often impaired.

*Cyanosis.*—In the *Gazette Médicale* for February, 1845, we find detailed at great length, a singular case of malformation of the heart in a child nearly seven years of age, who for five years had been affected with cyanosis. On dissection there were found the rudiments, certainly, of the four cavities of the heart, but only one auricle and one ventricle were efficient for carrying on the circulation.

Another case of cyanosis is related by M. F. Aran,\* and is valuable from all the stethoscopic signs having been accurately noted during life. After death, there was found narrowing of the pulmonary artery, and a communication between the two ventricles.

An interesting variety of heart-disease has been described by Dr. Macdonnell.† It was first noticed by Dr. Graves, in his clinical lectures, who had seen it in three instances; and since then two more cases have been seen by Dr. Macdonnell. Violent functional palpitations of the heart occur in the complaint, but the region of the heart presents nothing abnormal on auscultation or percussion. The two striking peculiarities in this affection are an enlargement, to a great degree, of the thyroid gland, giving to it the thrill under the hand of aneurismal varix, and a peculiar prominence of the eye-balls, causing a wild and ferocious aspect, though without any pain, dimness of vision, or other inconvenience. Rest and palliative treatment have afforded relief, but have not effected a cure.

*Abscess of the Heart.*—Of this rare disease an instance occurred in the practice of Dr. Chambers, of Colchester.‡ The symptoms during life that may perhaps be referred to the disease in question were epileptic convulsions and great distension of the jugular veins. An abscess, containing two ounces of pus, was found deeply

\* *Lancet*, 1844, p. 501.

† *Dublin Journal of Medical Science*, May, 1845, p. 200.

‡ *Lancet*, July, 1844, p. 557.

embedded in the substance of the heart, and extending from auricle to auricle around the apex of that organ.

In a case of partial inflammation of the heart's substance, described by M. Gintrac, of Bordeaux, matter formed in the parietes of the left ventricle, and burst into the pericardium.\*

Two very remarkable instances of death from rupture of the heart's fibres, and of the blood-vessels that ramify on its surface, have recently occurred in Edinburgh. One of these cases is probably well known, from the high professional attainments of the distinguished individual whose life was thus suddenly terminated. I allude of course to Dr. Abercrombie.† A rupture of the fibres of the heart, about half an inch in length, was found on the posterior surface of the left ventricle, but the fissure did not extend into the cavity of the organ. The coronary arteries had undergone atheromatous degeneration, and one of the coronary veins presented an open orifice of considerable size. Dr. Abercrombie's death was probably instantaneous; but in the other case, that of a lady aged 75, and related by Dr. Maclagan,‡ the patient survived for about an hour after the symptoms first showed themselves, and the appearances, on dissection, were very similar to those recorded above.

Dr. Duncan's case of fatal hæmorrhage, occasioned by false teeth impacted in the œsophagus,§ and perforating the aorta, is curious and rare, but does not present any circumstances of peculiar interest.

Death from perforation of the aorta does not always immediately ensue, but much of course depends upon the resistance offered to the flow of blood by the adjacent parts, which in this instance were soft and yielding. Dr. Peacock, of Edinburgh,|| and Mr. Tripe, of London,¶ have each recorded instances where an aneurism of the ascending aorta burst into the right ventricle. The disturbance of the balance of the circulation was, as might be expected, very great. In one case the patient seems to have lived only

\* *London and Edinburgh Journal of Medical Science*, July, 1844., p. 621.

† *Ibid.*, December, 1844.

‡ *Ibid.*, June, 1845.

§ *Northern Journal of Medicine*, April, 1844.

|| *London and Edinburgh Journal of Medical Science*, January, 1845, p. 16.

¶ *Lancet*, 1845, vol. i., p. 221.

eighteen hours after the accident; in the other, he probably survived for three or four days.

The formation of polypiform concretions in the heart, towards the period of dissolution, has been discussed by M. Aran,\* but he does not elucidate the subject by informing us that, in the case he describes, the blood had probably, during the few days preceding death, become so modified by pre-existing disease, that it acquired a great tendency to coagulate in the cavities of the heart.

Little advance has been made during the past year in the diagnosis or treatment of heart-disease in general, but we may refer to the admirable lectures of Dr. Bellingham,† and of Dr. Williams,‡ for excellent rules in these respects, unaccompanied by any predisposition to peculiar theories.

Digitalis has been strongly recommended in heart-disease by Dr. Munk,§ but Dr. Henderson has pointed out that in one species of heart complaint it would be dangerous, namely, in a patent state of the aortic orifice, which has been so admirably described by Dr. Corrigan. In such a case, if the system be placed under the influence of digitalis, the heart will become less and less able to expel the blood regurgitated into its cavity, and its hypertrophied and dilated condition will be necessarily increased. Dr. Munk ascribes the conflicting opinions regarding the efficacy of digitalis to its injudicious and indiscriminate administration. As a sedative he has found the tincture most efficacious, while the diuretic action of the drug is best promoted by the infusion.

As prophylactic of heart-disease during and after rheumatic affections, Dr. Furnivall|| strongly recommends treatment by alkaline preparations; and he asserts that he has hardly ever seen that organ become diseased when the rheumatism had been subdued by alkalies alone.

*Pericarditis.*—Dr. S. Scott Alison¶ has called our attention to the frequent occurrence of pericarditis after and during scarlatina,

\* *Archives Générales de Médecine*, August, 1844, p. 461.

† *Dublin Medical Press*, February, March, and April, 1845.

‡ *Medical Times*, April, May, and June, 1844, *passim*.

§ *Guy's Hospital Reports*, October, 1844.

|| On "Diseases of the Heart;" London, 1845.

¶ *London Medical Gazette*, February 21, 1845, p. 664.

a complication which has scarcely been noticed by former writers. In two instances the affection of the pericardium seems to have occurred early in the disease, though, from the history of the cases this is somewhat doubtful; and Dr. Snow expressly states his conviction that pericarditis after scarlatina supervenes only as a consequence of the renal disorders well known to prevail in that disease. The question of the relative importance of these two secondary disorders in scarlatina will, we hope, receive further elucidation.

*Arteries.*—Canstatt and Oesterlen\* both maintain that we cannot depend upon mere depth of colour as a proof of inflammation of the inner coats of the blood-vessels. Oesterlen, indeed, expressly asserts that the condition of the blood where this deep colouring is observed in the coats of the arteries, is the very reverse of inflammatory.

In his excellent review of the present state of our knowledge of heart-disease, Valleix† agrees with Andral and Bizot, that the atheromatous, cartilaginous, and bony masses, or plates, occurring on the surface of the heart and arteries, are not of inflammatory origin.

#### DISEASES OF THE CHYLOPOIETIC SYSTEM.

*Malignant Sore-Throat.*—An accidental circumstance, where tartar emetic had been given by mistake to a child labouring under this disease, impressed Dr. J. Arnott with a high opinion of the efficacy of emetics in such cases.‡ This mode of treatment has been tried in croup also, with success; the great point seems to be to keep up severe vomiting for a long period together. In strong children and adults this may fearlessly be practised, but in weak individuals it should surely be cautiously employed.

In cynanche tonsillaris Dr. B. Morris has used guaiacum with very satisfactory results.§

*Dyspepsia.*—Dr. Chapman, of Philadelphia, has strongly depicted the evil effects of American habits on the functions of the stomach.

\* Kleiner's *Repertorium*, May, 1844, p. 27.

† *Archives Générales de Médecine*, September, 1844, p. 50.

‡ *London Medical Gazette*, January 10, 1845, p. 475.

§ *London and Edinburgh Journal of Medical Science*, November, 1844, p. 949.

The rapid bolting of unmasticated food, so general at American tables, the excitement of commercial speculations, and the immoderate use of tobacco and ardent spirits, all combine to render dyspepsia the most common of transatlantic ailments. Most serious effects on the nervous system are shown by Dr. Chapman to result from the immoderate use of tobacco alone, by persons otherwise perfectly temperate. Dr. Chapman's treatment of dyspeptic cases does not differ from that pursued in England, and confirms the now generally received opinion, that the management of dyspepsia can be subjected to no positive rules, and that most remedies are only palliative unless a total change is effected in the diet and habits of the patient.

Pain in the region of the stomach arises from so many different causes, that it requires the utmost discrimination in the treatment. Neuralgic pain may simulate irritation of the mucous coat of that organ, and *vice versa*; and we have, besides, the long array of organic disease, cancer, scirrhus, and ulceration, wherein neuralgic pain of a more or less violent character does constantly occur. In true *gastralgie*, Dr. Strange\* recommends the well-known remedy, the tris-nitrate of bismuth, with half a grain or a grain of the extract of belladonna, and rubefacients and derivatives externally; while, at the same time, he judiciously insists on the necessity of altering and improving the too-generally debilitated constitution of the patient. In *gastralgie*, accompanied as it so often is by water-brash, Dr. Strange has found the above treatment peculiarly efficacious. Dr. Chapman speaks highly in praise of milk as a remedy in *gastralgie*, but he acknowledges that it sometimes accumulates in the stomach in large masses of a cheesy nature, and these may occasion terrible gastric oppression, and dangerous obstruction of the bowels.

Abdominal pulsation, so feelingly described some years ago by Dr. Hohnbaum, has been investigated by Dr. Nottingham,† and he confirms the general opinion that it arises from many different causes, as from tumours pressing upon the aorta, from aneurisms of that vessel, or, what is by far the most frequent, from nervous irritability. We ourselves have often thought abdominal pulsation to be connected with the arthritic diathesis.

\* *Northern Journal of Medicine*, July and December, 1844.

† *Medical Times*, February 22, 1845.

*Perforation of the Stomach.*—Several instances of this affection have been recorded within the last twelve months. In those described by Mr. Taylor,\* of Guildford, there was evidently ulceration of a slow insidious nature, which gradually perforated the coats of the organ. In that related by Dr. Lindberg, of Horsens, in Jutland,† and which occurred in a young female, a round opening, without any swelling, softened appearance, or redness, was observed in the anterior wall of the stomach. The patient had been indisposed for twenty-four hours with symptoms of peritonitis, when she suddenly sprang out of bed and died.

Rokitansky, who has so admirably described the various forms of ulceration of the stomach, always stated that individuals affected with this disease were frequently subject to violent cramp-like attacks in the stomach. I have sought in vain for any symptoms of this kind in the cases above mentioned.

A singular difficulty in respect to diagnosis occurred to Dr. Barlow in a case of perforating ulcer of the stomach. A large cyst or abscess had formed in the cavity of the peritoneum, and was connected, by means of the ulcer which had given rise to it, with the interior of the stomach. The contractions of the diaphragm in this case, causing the passage of air from the stomach to the adventitious cavity, and *vice versâ*, gave rise to many of the symptoms of pneumothorax with pulmonary fistula, such as amphoric resonance, metallic tinkling, etc., so that during life the pleural sac was considered to be the part chiefly involved.

*Hæmatemesis.*—Vomiting of blood from foreign bodies accidentally introduced into the stomach, is by no means of rare occurrence, but it is seldom that death ensues from such accidents. Mr. Dicken, of Ashby-de-la-Zouch, gives the history of a boy, aged ten years, who accidentally swallowed a halfpenny, when at play, and every effort to dislodge the coin from the body proved fruitless. For nearly a month no bad consequences ensued, but he was then suddenly attacked with hæmatemesis, which proved fatal on the third day. On dissection, a circular ulceration was found in the stomach, of exactly the size of the missing coin, and situated at some distance from the pyloric orifice. The coin had

\* *Lancet*, December, 1844, p. 369.

† *Bibliothek for Læger*, 1843, No. 4, p. 235.

traversed the whole length of the intestinal canal, and was found in the rectum; and the child, the day previous to his death, had distinctly felt the sensation of its being dislodged from the stomach, and of its passing through the intestines. This case confirms the opinion that very serious lesions may silently progress in the stomach without occasioning pain or any other symptoms.\*

*Enteritis.*—Perforations of the duodenum from inflammation are of rare occurrence. In an instance recorded by Dr. Engel, of Nakskov, the patient died, with all the symptoms of duodenitis, in thirty-three hours; and, on dissection, the duodenum was of the darkest crimson hue, almost black, and much bloody fluid had flowed into the cavity of the peritoneum from several perforations in the duodenum and upper part of the jejunum.†

*Intus-susception.*—A remarkable case of this kind, involving a difficult question of medical jurisprudence, is described by Dr. Tinniswood, of Carlisle. A boy had been kicked violently on the abdomen by one of his companions; he went to bed in his usual health, but during the night, symptoms of internal strangulation came on, and death rapidly ensued. On dissection, the obstruction was found to have been caused by a band of lymph, firm and old, passing from one portion of the intestines to another, and through this aperture twenty-six inches of the ileum had insinuated itself, and had been strangulated and become gangrenous, as in hernia. Dr. Tinniswood justly regards the internal hernia, (*Innere Darm einschnürung* of Rokitansky, second form,) as having existed for some time; but he thought that the blows on the abdomen had probably caused inflammation and gangrene of the incarcerated portion.

*Peritonitis.*—The old doctrine, that lumbrixi and other worms may perforate the intestine and cause fatal peritonitis, is adhered to by an excellent pathologist, Dr. Svitzer, of Copenhagen; and we confess that in the case which he records there is much that corroborates this opinion. Lumbrixi are very apt to pass into the peritoneum through any ulceration in the intestines, and then their

\* *London Medical Gazette*, April 11, 1845, p. 886.

† *Bibliothek for Læger.*, 1844, No. 1, p. 200.

contact with the peritoneum would probably occasion dangerous inflammation. An abstract of this case is given in the *London and Edinburgh Journal of Medical Science* for June, 1845.

The most important contribution to the subject of peritonitis has been Dr. Spittal's essay on friction-sounds audible in that malady.\* Laennec, we believe, has the honour of first noticing this symptom, as a means of diagnosis; but as the observations of that great man, alluded to by M. Piorry, did not occur in his printed works, the subject has remained for a long time unnoticed by the profession, or at least did not receive that degree of attention to which it is entitled. Dr. Spittal believes the mechanism by which the friction vibrations are produced to be of three kinds.

1. The respiratory movements, chiefly of the diaphragm, but also the action of the abdominal muscles; the vibrations being synchronous with these movements, though sometimes only detected during inspiration.

2. Pressure by the hand on the abdominal parietes.

3. The peristaltic motion of the intestinal canal.

By any of these means friction-sounds may be produced, if exudation of lymph has taken place on the peritoneal surface; but Dr. Spittal thinks that such sounds may be heard even before that event, when the peritoneal surface is dry at the commencement of the disease.

*Perityphlitis.*—Our knowledge in respect to this disorder has been increased by two valuable papers, by Dr. William Sellers† and Dr. Paterson.‡ Dr. Sellers contrasts these cases with the iliac abscesses occurring after delivery: he considers the seat of the malady to be in the cellular tissue, between the fascia of the iliacus internus and the coats of the cæcum. This fascia forms a barrier to the evacuation of the purulent matter externally, and therefore the cæcum, two thirds of which are uncovered by peritoneum, usually gives way in preference to any other part. Dr. Paterson, however, seems to hold an opposite opinion, at least with regard to the case related by him, where he considers the abscess originally to have formed in the vermiform process of the cæcum, and

\* *London and Edinburgh Journal of Medical Science*, May, 1845, p. 345.

† *Northern Journal of Medicine*, July, 1844.

‡ *Dublin Journal of Medical Science*, January, 1845, p. 412.

subsequently to have occasioned the collection of a large quantity of pus in the surrounding cellular tissue. The abscess burst suddenly one day, on the patient straining to evacuate the bladder, and caused fatal peritonitis. A small concretion, consisting of hair and mucus intimately mixed, was found in the peritoneal cavity; and Dr. Paterson suggests that this had been originally impacted in the appendix vermiciformis, from whence the inflammatory symptoms originated.

*Constipation.*—Small doses of strychnia, with a view of restoring the peristaltic action of the bowels, have been successfully administered by Mr. Small, of Boston,\* in a case of most obstinate constipation, which had resisted all other remedies for the space of ten days.

Dr. Barlow† has recently noted a symptom which, if it prove to be correct, will be of great importance in forming an exact opinion as to the seat of obstruction of the bowel. In cases where the obstruction is situated high up in the intestinal canal, nearer to the stomach, little or no urine, says Dr. Barlow, will probably be secreted, as a very small quantity of fluid can pass into the intestines, so that the supply by absorption to the kidneys will necessarily be reduced. The contrary, however, will be the case if the obstruction occur in the colon.

*Liver.*—The subject of hepatic disease has been investigated at great length by Professor Bang, of Copenhagen, in a most elaborate report, which we can only regret is too diffuse for analysis here.‡

Dr. Allnatt§ has treated of the diagnosis and cure of neuralgia hepatica, and has especially called our attention to the decidedly paroxysmal character of the disease, the patient enjoying during the intervals perfect immunity from pain, which is never the case in hepatitis. Moreover, in hepatic neuralgia, pressure gives relief during the utmost height of the paroxysm, and the dyspeptic symptoms so prominent in liver-disease are wanting, or but slightly marked, in neuralgia of that organ. Dr. Allnatt has seen a patient

\* *Medical Times*, February 2, 1845, p. 442.

† *London Medical Gazette*, May, 1845.

‡ *Bibliothek for Læger*, 1844, No. 1.

§ *London Medical Gazette*, March, 1845, p. 796.

attacked with violent hepatic neuralgia on the cessation of tic douloureux in the face; and he observed in this instance a frequent metastasis of the malady to its former seat, and then back again to the liver. Mercury and carbonate of iron aggravated the complaint, which yielded however to gentle purgatives combined with colchicum, ipecacuanha, and hyoscyamus.

*Jaundice.*—Dr. Corrigan\* has described a form of jaundice which has hitherto not been generally recognised, or at least distinguished from the other conditions in which this malady occurs. The disease usually sets in suddenly: in twenty-four or thirty hours the patient may be jaundiced all over; but the pulse continues regular, the tongue clean, the appetite undiminished, the white stools and the urine loaded with bile, being the only indications of deranged health. If, however, the slightest symptoms of head-affection now appear, as coma or delirium, no effort of art can save the patient, nor can we after death discover the slightest indication of disease in any organ of the body. Of all remedies hitherto employed, the most efficacious have been found to be emetics. Dr. Corrigan gives half a drachm of ipecacuanha every second night till the jaundice disappears.

## DISEASES OF THE URINARY SYSTEM.

*Diabetes.*—From the present chemical tendency of medical investigation, it is naturally to be expected that this hitherto imperfectly understood disease will have received a considerable share of attention. No new theories, however, regarding the possible cause of the malady have been promulgated, excepting those of Dr. Watts;† nor can we characterise his doctrines as perfectly novel, since it has long been maintained that the primary cause of diabetes resides in the digestive, and not in the uropoietic organs. Dr. Watts, however, has added several important facts to our knowledge of diabetes, which disease he believes to be marked at its commencement by the presence of lithates in the urine, and by a sub-inflammatory condition of the gastric mucous membrane, which

\* *Medical Times*, January 25, 1845.

† *Lancet*, April 19 and 27, 1845.

renders the digestive organs unable to azotize the saccharine and the oleaginous secondary principles, and produces a great tendency to the accumulation of fat. This obesity is subsequently followed by diabetes mellitus, properly so called, where the digestive process affords a low kind of sugar, but is incapable of accomplishing its further assimilation.

The advantages of a residence in a hot climate in the cure of diabetes have been strongly urged by Dr. Keith Imray, who informs us that this disorder is almost unknown within the tropics, and that of several patients who, at his recommendation, had resided in the West Indies, after suffering in Scotland from well-marked diabetic symptoms, not one had fallen a victim to this disease.\*

Mr. Moore, of Queen's Hospital, Birmingham,† has proposed to employ the liquor potassæ as a test for sugar in the urine. On boiling liquor potassæ with diabetic urine, no change of colour is at first perceived, the glucic acid which is then formed being colourless; but by the continued action of heat this acid is changed into the melassic, which gives a highly characteristic rich claret colour to the fluid. Mr. Moore does not coincide with Dr. Golding Bird in that gentleman's high estimate of the value of Trommer's test for diabetic urine, as he finds that the slightest variation in the quantity of the chemical re-agents employed therein, alters considerably the colour produced; and Mr. W. T. Gairdner adds strong testimony to the same effect.‡ Mr. Gairdner believes that the precipitate thrown down by Trommer's test is not uncombined protoxide of copper, but that both in diabetic and in healthy urine, a similar precipitate may result from a combination of the oxides of copper with various animal substances occurring in the urine. Mr. Gairdner prefers, to all others, the fermentation test with yeast, aided by the microscope, to detect the conervoid vegetations.

*Albuminuria.*—The opinions of Dr. Corrigan§ on this disease are remarkable, inasmuch as he believes it to be analogous to the

\* *Edinburgh Medical and Surgical Journal*, January, 1845, p. 65.

† *Lancet*, September 14, 1844, p. 751.

‡ *London and Edinburgh Journal of Medical Science*, July, 1844, p. 565.

§ *Medical Times*, April 5, 1845.

disorder he has so well described as cirrhosis of the liver. The first stage of the disease is one of hypertrophy, caused by the deposit of lymph in the intervening cellular tissue, and not in the tubular substance of the kidneys. The second stage is one of contraction, as in cirrhosis. The former condition Dr. Corrigan considers curable; the latter, as perfectly beyond the reach of art, and characterised by the low specific gravity of the urine, which, in the earlier stage of hypertrophy, is scarcely altered in that respect.

M. Fourcault and Mr. G. Ross\* believe the presence of albumen in the urine to depend mainly upon diminished activity of the cutaneous perspiration. Albuminuria was certainly produced in M. Fourcault's experiments of totally suppressing in animals all evaporation by the skin; and Mr. Ross adds, that should ascites, or effusion into any serous cavity take place, the quantity of albumen in the urine will disappear or will be greatly diminished. These observations of the authors above named have opened a wide field for further enquiry.

A curious instance of the effect of cantharides on the urinary bladder has been recorded by M. Morel Lavallée.† Four patients, to whose loins or chest large blisters had been applied, suffered severely from all the symptoms of strangury, and passed subsequently a quantity of false membranes from the urethra.

Mr. Parker, of Liverpool,‡ describes a case of calculus of the bladder, where the nucleus of each concretion was formed by a minute hair, exactly similar in structure to the hairs of the scalp. The point of the hair was imbedded in the calcareous matter of the calculus, while the bulb was distinctly visible at the other extremity; whence Mr. Parker concludes that these hairs could not have been introduced from without, but that they had grown on the inner surface of the bladder, and subsequently became detached from their position by the weight of the accumulating calculus.

Of Dr. Golding Bird's admirable work on urinary diseases, it is unnecessary here to speak, as the greater part of the observations in

\* *Lancet*, August 3, 1844.

† *Archives Générales de Médecine*, August, 1844, p. 517.

‡ *Edinburgh Medical and Surgical Journal*, July, 1844, p. 152.

this book has previously appeared in one of our medical journals; we may, however, particularly recommend the chapter on the oxalic acid diathesis, as highly deserving the attention of the profession.

## DISEASES OF THE REPRODUCTIVE SYSTEM.

*Dysmenorrhea.*—This disorder is considered by Dr. Rigby\* to be almost always intimately connected with appreciable derangement of the functions of digestion and nutrition, and that it is often a symptom of the gouty or rheumatic diathesis. In such cases the urine is generally loaded with lithates, and the functions of the skin are also impaired. Dr. Rigby recommends in such cases the employment of colchicum, guaiacum, or iodine, and praises the extract of taraxacum, as having great power in restoring the dormant action of the liver.

*Chlorosis.*—Of the researches of Becquerel and Rodier on the condition of the blood in this disease we have already spoken. They seem to prove that chlorosis should be distinctly separated from anaemia, with which it has hitherto been so often classed; for while in the latter condition the mass of the blood is impaired, and probably diminished in quantity, in chlorosis we find that the contrary state may and does frequently exist; that there is an absolute increase of the mass of the blood—an absolute plethoric condition of the system. These conclusions are corroborated by the testimony of M. A. Duchassaing,† who states that the majority, or at least a considerable proportion, of the congestions of various organs occurring in chlorotic females, are caused by an aqueous plethora, which is relieved by venesection and purgatives. Dr. Ashwell has judiciously recommended the iodide of iron in the chlorosis of strumous females.

*Ovarian Disease.*—We do not here propose to enter upon the subject of ovarian disease and the recent controversy regarding the extirpation of ovarian cysts, as this lies more properly within the province of surgery. In reference, however, to inflammation

\* *Medico-Chirurgical Review*, July, 1844.

† *Journal de Médecine de M. Beau*, December, 1844.

of the ovaries, M. Chereau corroborates the opinion, that inflammation of the left ovary is more frequent than that of the right; and he accounts for this fact by the greater proximity of the left ovary to the rectum, in which faecal matter is so apt to accumulate in females who neglect the state of their bowels. In forty cases occurring in his own practice, or recorded in various journals, M. Chereau found that ovarian disease had occurred twenty-five times on the left side, eleven times on the right, and four times on both sides.\*

### DISEASES OF UNCERTAIN SEAT.

*Gout*.—It is suggested by Mr. Alexander Ure,† that the uric acid which exists and accumulates in the urine of individuals of a gouty habit, may become urate of soda through the medium of the serum of the blood, at the ordinary temperature of the human body, and that the phenomena of gout may thus arise from a depraved condition of the blood, caused by its admixture with the above salt. As a cholagogue cathartic in gout and jaundice, Mr. Ure recommends the sulphate of manganese, which exists naturally in the anti-arthritis waters of Carlsbad and Marienbad, in Germany. During the paroxysms he advises that the affected limb should be bathed with acetic ether, or, better still, with pure naphtha, by which the pain and swelling are usually much relieved. Mr. Ure has also tried the silicate of potash (the ancient liquor of flints) in tophaceous concretions of the joints, and apparently with favourable results.

Dr. De Bourge, of Rollot, recommends the extract of the flowers of colchicum in gout, and that it should be given in combination with aconite, which he believes to have a peculiar efficacy in averting the deleterious action of colchicum on the digestive organs.‡

*Rheumatism*.—Dr. Wilson § has strongly reprobated the error of considering rheumatic fever as a local disease, insisting with many, we might say most, of the pathologists of the present day, that it is as much a constitutional disorder as any of the exanthemata.

\* *Journal des Connaissances Médicales*, November, 1844.

† *London Medical Gazette*, November, 1844, p. 190.

‡ *Gazette Médicale de Paris*, 1845, No. 8.

§ *Lancet*, November and December, 1844.

According to this doctrine, metastasis does not really take place in rheumatism; for as all the blood circulating through the body is in an unhealthy condition, deposits or congestions may anywhere occur, but they are found most frequently in the heart. Thus the joint-fever of rheumatism, to use Dr. Wilson's own words, becomes a heart-fever. In some cases of most extensive heart complication, the pulse at the wrist remained unaltered. In one instance the heart was drowned in serum, its cavities were plugged with fibrin, hydrothorax co-existed with apoplexy of the lungs, yet no irregularity existed in the radial pulse. Could not the stethoscope and percusion have revealed some of these lesions during life? Dr. Wilson never once refers to these means of diagnosis. The cerebral complications of rheumatic fever are believed by Dr. Wilson to be entirely sympathetic of disease of the heart or lungs, and that therefore antiphlogistic treatment directed to the head is of no avail. The plan of treatment so much employed in rheumatic fever—that by strong doses of calomel and opium—he deprecates as injudicious,—“as a rude empirical practice, which seldom succeeds, and which, failing of success, is often most injurious to the patient.” Colehicum, digitalis, and venesection, he places nearly in the same unfavourable light. Dr. Wilson's practice in rheumatism therefore becomes one of extreme simplicity. Of all remedies, the most efficacious, in his opinion, is the hot-air bath, to promote perspiration, while internally he administers salines, with an excess of a carbonated alkali, and trusts to rest and perfect quietude of body and mind for completing the cure.

Dr. Henry Bennet has added to his former observations of the efficacy of large doses of nitrate of potass in acute rheumatism. The sulphate of quinine, so highly extolled by M. Legroux, he does not consider entitled to much attention; and this is corroborated by Mr. Barnes, who remarks also on the facility with which doses of nitrate of potass are supported by rheumatic patients, to an amount which would prove seriously hurtful to a healthy individual.

Dr. Neligan has administered hemlock, with decided advantage, in rheumatic affections, both sub-acute and chronic, particularly when attended with severe pain, neuralgia, and senile gangrene.

Dr. Williams's lectures on rheumatism are also deserving of an attentive perusal.

*Skin Diseases.*—The “trâtement Arabique,” or Arabian formula for skin diseases of a chronic kind, whether they be of syphilitic origin or not, has been detailed and briefly commented on by Mr. Dangerfield.\* The formula is much too long for insertion, and, like the decoctum Zittmanni, appears to be unnecessarily complicated.

We may here also notice the epidemic of erysipelatous fever which occurred in Vermont and New Hampshire in 1842-43. It is described by Drs. Hall and Dexter as having been of an exceedingly malignant character; and the disorganization of the cellular tissue in some cases proceeded so far as to separate the muscles from the bones. Three medical men are reported to have fallen victims to their zeal in examining bodies after death. The treatment, as in most new and severe epidemics, appears to have been various and uncertain.

In addition to the notices of the spedalskhed, or Elephantiasis Græcorum, by Dr. Danielsen, of Bergen, we have this year a more detailed and extended history of this remarkable malady by Dr. Boeck, of Kongsberg, in Norway.† This gentleman was sent by the Norwegian Government to investigate the disorder in its present chief habitat in Europe,—the western coast of Norway. The entire number of individuals affected with spedalskhed in Norway amounted to 659; and it is curious, that in Bergenstift the disease is considered by the people to be non-contagious, while in Nordland, in the South of France, and in Greece, it is regarded as highly communicable from one person to another. Now, in Bergenstift, where the affected continue their relations to society and to their families, the disease prevails in the proportion of 1 in 511; while in the more inhospitable climate of Nordland, where the miserable leper, as of old, is thrust forth from the fellowship of mankind, it only occurs in the proportion of 1 to 947 of the population; and in Greece, where a similar fear of contagion exists, only as 1 in 2505.

For a detailed account of this singular malady we must refer to the excellent periodical from which this is quoted, though there also, many facts are omitted from want of room, which give great interest to the original.

\* *Lancet*, July 13, 1844.

† *British and Foreign Medical Review*, October, 1844.

Dr. Skae\* and Mr. Wills† concur in believing that the sibbens of Scotland is identical with venereal condyloma. Thus the general identity of all the diseases which have been assimilated to the old leprosy receives an additional confirmation. The most distinguished physicians of the countries where these maladies prevail, agree in ascribing to the radesyge and spedalskhed of Norway, and to the liktraa of Iceland, a probable though distant syphilitic origin.

### F E V E R.

Much has been lately effected in this country towards the obtaining more complete statistical accounts of the prevalence of fever in our manufacturing towns, as well as in our country districts. But it must be acknowledged that, as yet, our continental, and especially our German brethren, by the excellent arrangement of the whole country being parcelled out into districts, each under the superintendence of responsible medical officers, possess much more perfect machinery for the collecting of reports upon the sanitary condition of the people. How much more complete, for instance, would be the history of the curious epidemic fever which prevailed in several parts of Scotland in 1843, could it have been possible to obtain from every town and district where the fever raged, a detailed history of the rise, progress, and termination of the malady.

Some additional observations upon this epidemic have been published by Dr. Smith,‡ who does not agree with Dr. Cormack in assimilating this disorder to the yellow fever of the West Indies. Vomiting in the Scottish fever was a symptom seldom observed, and the still more characteristic black vomit (*vomito prieto*) was never seen.

The most valuable contribution to the pathology and treatment of fever during the past year is undoubtedly that of Dr. Davidson, of Glasgow,§ than whom few, if any, have enjoyed better opportunities of studying this scourge of great cities in its most

\* *London and Edinburgh Journal of Medical Science*, July, 1844.

† *Ibid.*, April, 1844.

‡ *Edinburgh Medical and Surgical Journal*, July, 1844.

§ *London and Edinburgh Journal of Medical Science*, December, 1844, p. 997.

malignant forms. Dr. Davidson regards the poison of typhus as resident in the blood itself, and does not believe that we possess any means of expelling it at once from the system. We must wait, therefore, for the spontaneous subsiding of the morbid action according to its own determinate laws, and content ourselves with watching and relieving urgent symptoms, and, above all, keeping up the vital strength of the system till the poison is eliminated. We can hence naturally conclude that Dr. Davidson is no friend to the lancet in typhus fever; and he justly argues that it tends still further to weaken the debilitated inhabitants of large cities, who are the most frequent subjects of the disease. Real inflammation, he states, rarely occurs in typhus, though congestion of various organs is of frequent occurrence. Emetics are insufficient to check the course of typhus, though they may be useful in the catarrhal forms of that malady. Much relief is afforded by sponging the surface of the body with tepid water; and diaphoretics—chiefly small doses of ipecacuanha with tartar emetic—have also a beneficial effect. Mercury is of no value as a specific in typhus, but it may be useful as a means of regulating disordered secretions. The sheet-anchor of Dr. Davidson's practice is wine, which he administers freely in all cases of weak pulse and exhausted condition of the system. In the very earliest stage of fever, while there is great excitement, wine should be withheld, but at a more advanced period, even though pneumonia should be present, wine is necessary to support the failing energies of the patient. To the use of opium in typhus fever Dr. Davidson is unfavourable, save in small doses to check the exhaustion of diarrhoea. On the other hand, we find Mr. Burgess, of London, recommending large doses of calomel and the cold affusion in the worst cases of typhus.

The history of several epidemics of typhoid fever, occurring in the Austrian capital, has been recorded by various members of the Vienna Medical Association.\* The details given confirm the general opinion, that bad sewerage and deficient ventilation are some of the chief causes of any form of typhus; and we observe, too, that the most favourite treatment was that by powdered alum, in doses of from two to five grains every hour. This was given under all circumstances, partly, it seems, as an antiseptic, and partly to arrest

\* *Transactions of the Vienna Medical Association*, reviewed in the *British and Foreign Medical Review*, January, 1845.

the diarrhoea, so frequent, if not constant, in the abdominal complication of typhus in Germany.

In the typhoid fever of children, Rogée\* has found that the temperature of the body may form a means of diagnosis between this disorder and simple enteritis. In the latter disease the temperature is lower than in abdominal typhus, in the proportion of  $37\frac{9}{100}$  to  $40\frac{1}{100}$ .

As a specimen of a singular train of investigation on the part of our German brethren, we may here refer to the opinions of Dr. Beer, of Vienna,† of the relation of vaccination to typhoid fever. Dr. Beer asserts that ileo-typhus (*i. e.* that with the abdominal complication) was unknown during the prevalence of small-pox, and was not, indeed, described till the year 1810 or 1811. (We thought that Rœdcrer and Wagler wrote before this period!) All the sufferers from typhoid fever, according to Dr. Beer, have been vaccinated individuals; those who have been inoculated after the old method invariably escape, for small-pox and typhoid fever mutually destroy one another. The origin of this doctrine is probably the theory once maintained by Schönlein, that abdominal typhus is an intestinal exanthema.

*Intermittent Fever.*—The general effects of malaria at a distance have been illustrated by Dr. Robertson,‡ of Northampton, and Dr. Parkin, of Woolwich; the latter gentleman states, that the effects of malaria near Woolwich are often first observed on the summit of Shooter's Hill, from whence they gradually descend, becoming more malignant in their character as they approach the sources of infection about the Plumstead marshes.

Dr. Robert Willis§ has advanced a perfectly new theory regarding the origin of intermittents, which, if correct, will essentially alter our theories and treatment of the disease. Dr. Willis denies altogether the existence of marsh miasm or malaria, which, in truth, the best chemists have never been able to detect, and maintains that the supposed influence is nothing more nor less than that of moist warm air—air excessively moist, considered in connexion with its own temperature and with that of the human

\* *Archives Générales de Médecine*, October, 1844, p. 154.

† *Kleinert's Repertorium*, March, 1844, p. 116.

‡ *Provincial Medical and Surgical Journal*, August, 1844, p. 342.

§ *London Medical Gazette*, July 12, 1844, p. 482.

body. The arguments by which Dr. Willis supports his theory are certainly ingenious, and the whole paper worthy of consideration.

M. Henri Gouraud questions the accuracy of M. Pierry's doctrine regarding the action of the sulphate of quinine on the enlarged spleen of intermittent fever. Professor Pierry stated two years ago, to the French Académie des Sciences, that if a gramme (sixteen grains and-a-half) of quinine dissolved in water, with a little additional sulphuric acid, were administered to a patient with enlarged spleen, the volume of that organ would be very sensibly diminished in the short space of forty seconds. The fact itself, as elicited by percussion, M. Gouraud does not deny; but he much doubts the inference drawn from it, and believes it to admit of a much more simple and rational explanation. He has found that an exactly similar result may be produced by a few drops of sulphuric acid in an equal quantity of water, or by lemonade, by wine and water, or even by simple distilled water. The apparent diminution of size of the spleen cannot, therefore, arise from a specific action of the quinine on that organ; and M. Gouraud thinks that a developement of gas takes place in the stomach when fluids are swallowed, by which the larger curvature is distended and the spleen forced downwards and backwards, beyond the reach of percussion. He has found the same phenomena to ensue in experiments on the healthy spleen as on that which was diseased.\*

*Scarlatina*.—In addition to what has been already said regarding the connexion of scarlatina with pericarditis, Dr. Corrigan has called our attention to a symptom in this malady of very dangerous tendency, and which he believes has not before been observed. This is a diffuse inflammation of the cellular tissue of the jaw and around the ear, which, however, must not be confounded with simple parotitis. It very frequently terminates in death, by sloughing of the cellular tissue and adjacent parts; and treatment by incisions generally hastened the unfavourable result.†

Rogée has found that the temperature of the body always is increased in scarlatina, but that the pulse and the quickness of respiration do not always correspond with the elevation of the temperature.‡

\* *Journal des Connaissances Médico-Chirurgicales*, December, 1844.

† *Medical Times*, May 10, 1845.

‡ *Archives Générales de Médecine*, October, 1844, p. 140.

Dr. Golding Bird\* has laid down some excellent rules for distinguishing between the dropsy of debility, (the asthenic dropsy of Dr. Willshire,) and that which arises also after scarlatina from really diseased kidneys. He believes that the more frequent occurrence of dropsy after mild than after severe attacks of scarlatina, may be explained by the supposition that where the eruption is slight, it is insufficient to eliminate the morbid matter from the system; the residuc, therefore, of the poison must be carried off by some other channel, and most naturally by the kidneys, whose capillaries then become dilated, and congestion ensues. The consequence of this is a double lesion of their functions; on the one hand, exudation of the albuminous element of the blood, and, on the other, retention of the nitrogenized products.

*Measles*.—Rogée found the temprature of the body less elevated in measles and variola than in scarlatina.

Dr. Cathcart Lees has described a severe form of laryngeal inflammation after measles; and in one or two instances the larynx, epiglottis, and pharynx, became covered with false membranes, as has been likewise noticed by Rokitansky.†

*Variola*.—The attention of pathologists has of late been much directed to the antagonism of small-pox towards other diseases, and especially in regard to the other exanthemata. Mr. Pittock, of Sellinge, relates a case of variola and the vaccine eruption occurring together; the former predominated, and the child died‡.

In reference to this subject, we have also the extensive researches of Legendre, who considers that it is now fully established, that if a child be inoculated with small-pox matter on the fifth day after vaccination, the variola will have only a local, and not a constitutional effect. Spontaneous variola is said, however, not to be subject to this law, though its intensity is greatly modified; and, indeed, M. Legendre inclines to the very probable supposition, that in the majority of such instances the poison of variola has been long present, undeveloped in the system. He states also, that weakly children under four years of age, incur great danger by being vaccinated after they have been exposed to the contagion

\* *Guy's Hospital Reports*, April, 1845, p. 131.

† *Dublin Journal of Medical Science*, September, 1844.

‡ *Provincial Medical and Surgical Journal*, August, 1844, p. 343.

of variola, for in such case vaccination seems to hasten the progress of small-pox, instead of allaying its dangers.

### FORENSIC MEDICINE.

Perhaps at no period in the brief history of English medical jurisprudence have more important questions been discussed in relation to this subject than during the past twelve months. The cases of Belaney and of Tawell, the Scottish trials for poisoning by arsenic, &c., have originated such a mass of essays and papers on toxicology, that it is only possible here within the limits of this Address to notice, in the briefest possible manner, the chief facts that have been established or impugned.

*Arsenic.*—We are, perhaps, now scarcely justified in placing this mineral at the head of the list of poisons, as of late years death by its administration has become rare, in comparison to the increased frequency of poisoning by prussic acid.

A singular case of recovery after taking a very large dose of arsenious acid, at least two ounces, is related in the American journals; and the patient, a lunatic, did not only not lose his life, but actually recovered his senses! We must own that we feel considerable reluctance in giving to this story the credit that it claims.\*

The researches in reference to arsenic have been chiefly directed to the trial of antidotes, and to the detection of the poison in the body after death. Numerous additional instances have been recorded where the hydrated peroxide of iron proved successful in counteracting the effects of the poison. It seems also, from the case recorded by Mr. Argent, of Hinckley,† that if arsenious acid be swallowed with, or soon after, a full meal, its effects will be much retarded, and the matters previously eaten will probably protect for a time the coats of the stomach, and serve as a vehicle if the poison be ejected by vomiting. According to Dr. Krafft, large draughts of milk and water favour, instead of retard, the action of the hydrated peroxide as an antidote. The good effects of this preparation are also strongly corroborated by Dr. Flechner, who

\* *American Journal of the Medical Sciences: Lancet*, 1844.

† *Lancet*, 1844, vol. ii., p. 103.

gave it with great success, in small and gradually increasing doses, to five individuals, who had been poisoned by drinking water from a spring, in the immediate vicinity of which a quantity of cobalt ore, containing arsenic, had lain during the winter. Dr. Flechner also communicates the interesting fact, that it is a common practice among the Tyrolese and Styrian hunters, to swallow as much as two grains of arsenious acid, as a tonic and invigorant, before setting out on their long and perilous mountain journeys.\*

Ollivier d'Angers confirms the truth of Orfila's observations, that arsenic, when existing in the earth of church-yards, will not impregnate bodies buried therein; but that, on the other hand, bodies containing arsenic will, as they decay, impart this poison to the surrounding soil. The presence of arsenic normally in various soils presents a most interesting subject for future investigation.

*Tests for Arsenic.*—Fresenius, of Giessen, maintains that the only certain and satisfactory test of arsenic is that of reducing it to the state of sulphuret; and he has also suggested a novel and, we think, more satisfactory mode of decolorization by chlorine. A good modification of Marsh's test, by which none of the arsenic can be lost, and the troublesome frothing of the animal matters in the apparatus is obviated, has been communicated by Berzelius to the Prussian Government. Other modes of testing the presence of arsenic have been proposed by Dr. Ayres, and by Mr. Lethby.

*Opium.*—Mr. Lethby† has succeeded in detecting the presence of opium in the stomach by the usual tests, not less than twelve days after death.

The most ample researches of recent date on poisoning by this drug are those of Mr. Taylor.‡ From this gentleman's experiments it appears that iodic acid is the most delicate test of the salts of morphia, when unmixed with other matters, but it is not applicable when the poison is mixed with coloured organic fluids; moreover, that in many cases of child-poisoning by opium, such as that on which Mr. Taylor has founded his observations, we cannot expect

\* *Transactions of the Vienna Medical Association*, in the *British and Foreign Medical Review*, January, 1845.

† *Lancet*, 1844.

‡ *Gay's Hospital Reports*, April, 1845, p. 269.

to find traces of the poison in the stomach or intestines, as the quantity sufficient to destroy the life of an infant will yield too little morphia to be detected by any known re-agents. Meconic acid may, however, be discovered in many cases where no morphia can be found, as under favourable circumstances the sesqui-chloride of iron will show the meconic acid contained in the 160th part of a grain of opium.

*Hydrocyanic Acid.*—Of all the means by which the suicide terminates his life, or the murderer destroys his victim, this deadly poison is perhaps the most efficacious; and in this country we have obtained an unenviable notoriety for its use. It is only, indeed, in Great Britain, where every shop-boy has access to, and can distribute, the most deleterious drugs, where no law exists regarding the sale of poisons, that hydrocyanic acid can be obtained by any one desirous of terminating either his own life or that of another.

The two most remarkable criminal trials that have recently occurred in relation to this poison are those of Belaney and of Tawell. In the former case, if we regard the medical testimony alone, its confined and uncertain nature, and the evidently contradictory opinions maintained by divers witnesses, we might say that the accused certainly could not have been condemned upon that portion of the evidence; and we may also assert, that much of what was then advanced has been contradicted by subsequent cases and investigations. Since the trial of Belaney many circumstances have concurred to prove that the death-shriek, to which some of the medical witnesses so confidently spoke on that occasion, does not necessarily occur, nay, it is doubtful if it ever takes place in the human subject. Even in animals it has been found that many die without uttering any shriek whatsoever. “The occurrence of a cry or shriek, therefore,” says Mr. Taylor, of London, “in poisoning by this acid, must be regarded as a purely accidental result, to which no medico-legal importance can be attached.”\*

Another, and a still more important question is, as to the degree of volition enjoyed by individuals after swallowing prussic acid in poisonous doses. This question was agitated on occasion of a trial some years ago; and the recent cases have, we think, sufficiently

\* “Report on Toxicology,” in the *British and Foreign Medical Review*, October, 1844; and Case by Mr. Hicks, of Newington, in the *Medical Gazette*, April 11, 1845, p. 898.

proved, that many acts which require deliberation and some considerable period of time, may be performed before the individual loses all consciousness. Thus, in the instance related by Mr. Godfrey,\* a gentleman swallowed half an ounce of prussic acid, and then walked deliberately fifty-five paces and down seventeen steps, into the druggist's shop, where he asked for more of the poison, before the fatal effects of the previous dose became manifest.

In one of two instances of death by prussic acid, which have occurred under our own observation in Newcastle, the suicide, an apothecaries' assistant, had swallowed an ounce of prussic acid, of great strength, and was apparently able afterwards to cork the bottle and thrust it through the hedge of the high road, on which he was found forty yards from the spot where the phial was subsequently discovered.

That death from prussic acid is not always attended with convulsions, as has been generally supposed, is partly proved by the quiet, composed attitude in which several suicides by this poison have been recently found, especially in the case of the double suicide recorded by Mr. Lethaby.† It is, however, ingeniously suggested by Mr. Taylor, that, as has been observed in animals, convulsions may occur with intervals of repose, and that they may take place, perhaps, before consciousness is so entirely lost as to prevent the sufferer from assuming a quiet aspect and position in death.

Another interesting question, which has been much agitated during the recent trials, is with regard to the amount of evidence afforded by the odour of prussic acid in the body after death. It was well ruled by the judge, in the case of Tawell, that the presence of the odour was a proof of the poison being there; but that, on the other hand, its absence was no positive proof that the poison had not been taken. The stomach usually retains the peculiar odour longer than any other organ. In the case in question, no smell of bitter almonds could be detected in the body of Sarah Hart by most, if not by all, present at the dissection; yet the existence of the poison was distinctly proved by the appropriate tests.

Mr. Taylor considers the nitrate of silver and the Prussian blue tests as nearly of equal delicacy and value, and that by these means

\* Provincial Medical and Surgical Journal, September 25, 1844.

† Lancet, 1844, vol. ii., p. 336.

we can discover the presence of about one-fiftieth of a grain of anhydrous acid in the body. For general use, he prefers the Prussian blue test, because it is extremely difficult to obtain cyanogen from very minute quantities of the cyanide of silver.

With respect to the minimum quantity of hydrocyanic acid capable of producing death, we have one instance recorded where nine-tenths of a grain of anhydrous acid proved fatal; and it is probable that even half a grain might prove destructive to human life. All antidotes to prussic acid will, on account of the fearful rapidity of its action, prove but of inferior and relative value. The Messrs. Smith, of Edinburgh, have however stated, that the sulphate of iron, combined with an alkaline carbonate, is an efficacious remedy; and they corroborate this assertion by ingenious chemical formulæ, and by the result of their experiments on animals. Everything, however, seems to depend upon the rapid administration of the antidote.\*

Dr. Bull, of Hereford, has recorded a fatal case of poisoning by the essential oil of bitter almonds, which presents many points of interest, and where death was apparently produced by about fifteen or seventeen drops of the undiluted oil. It is much to be regretted that so dangerous an ingredient should be so carelessly employed in domestic cookery.†

*Phosphorus.*—Mr. Reedal, of Sheffield,‡ has narrated an instance of poisoning by phosphorus, where there ensued fatal inflammation of the cæcum and colon, as also of the brain and pleura. Although the boy had not taken any phosphorus for ten days previous to his death, we think Mr. Reedal perfectly justified in ascribing the event to its well known irritating properties.

The admirable remarks by Dr. Cowan, in the Address of last year, on the necessity of medical education for coroners, have been but too frequently verified by instances of the greatest ignorance and indifference to medical evidence, on the part of some of these functionaries, during the past year. Indeed, so long as the law remains in its present state,—so long as the ancient office of coroner is awarded to any but medical men,—we

\* *Lancet*, October 5, 1845, p. 54.

† *Provincial Medical and Surgical Journal*, September, 1844, p. 364.

‡ *Lancet*, September, 1844, p. 364.

must expect such scenes as have recently occurred, and remain patiently, and almost without hope, for improvement in this branch of forensic medicine. How far are we behind other countries of Europe in this regard! In almost all continental states each district has its superintending physician, whose duties are in part similar to those of the coroner of England, who must examine, either personally or by deputy, into every suspected case, and who must transmit to a higher court of medical and legal officials, a full report, not only of the circumstances attending the death, but, moreover, a complete history of the appearances observed on the *post-mortem* examination. In this country the coroner, himself, too often partakes of the vulgar prejudices against dissection; and should he be superior to these, he is in perpetual peril of rebuke, if not of absolute pecuniary loss, from the bench of magistrates, on presenting his quarterly accounts. We trust ere long that the absurd and mischievous condition of our English law respecting coroners will be ameliorated.

#### MATERIA MEDICA.

From the zeal and perseverance with which botanical researches are now prosecuted in tropical lands, and, on the other hand, from our daily increasing experiments in forming new products and combinations in chemistry, it is natural to expect that our *materia medica* will become considerably enriched.

*Bebeerine*.—Of perfectly new remedies we have but one or two properly so called. Dr. A. D. MacLagan, of Edinburgh, has introduced into practice the sulphate of bebeerine, the existence of which, as a vegetable alkali, was first pointed out by Dr. Rodier, of Guiana, where the bebeelu, or green-heart tree, is found. In its therapeutical effects this salt appears to be analogous to quinine, but it possesses, in addition, the very great advantage of not causing any febrile excitement in the system.\*

*Piscidia erythrina*.—Mr. Hamilton† has made some personal experiments on the narcotic effects of the *Piscidia erythrina*, or Jamaica dogwood, which has long been employed by the negroes in

\* *Edinburgh Medical and Surgical Journal*, April, 1845, p. 359.

† *London Journal of Pharmacy*, 1844.

that country for the purpose of narcotizing fishes. Mr. Hamilton swallowed, in the evening, an ounce of the alcoholic tincture, prepared from the bark of the root. He experienced a sensation of intense heat in the stomach, and then immediately fell into a sleep so profound, that, on awaking some twenty-four hours after, he still held in his hand the phial containing the residue of the tincture. The violent toothache, for which he had tried this remedy, had entirely ceased, and no injurious results ensued from the action of so powerful a narcotic.

*Quinine*.—M. Bourrieres has proposed the arseniate of quinine as a substitute for arsenious acid, in cases where that poison is exhibited as an anti-periodic. Further experiments on the valerianate of quinine have also been made by M. F. Devay. He has found it to be valuable in those cases where a combined tonic and sedative effect was required, as in low forms of fever, with nervous excitability; in intermittents of bad character; and in neuralgic and hysterical complaints.

*Cannabis Indica*.—The real virtues of this drug may still be considered doubtful, though much probably depends upon the goodness of the extract employed. It seems very probable that its efficacy is less in this country than in India, whether from the influence of a colder climate, or from the effects of the long sea voyage. But while we have the testimony of Mr. Donovan, and of so many others, in its favour, we must consider the extract of Indian hemp as worthy of a further trial in our *materia medica*.

### MESMERISM.

Our Address would not be complete did we not allude to the subject of mesmerism; but the brief space allowed forbids us to mention either fact or theory here, for the one requires detail, and the other arguments, to render it intelligible. During the past year this so called science has been subjected to the most searching and candid trials, by men as much distinguished for their talents as for their strict impartiality. We are bound to say, that in no one instance does mesmerism appear to us to have stood the test of real investigation; and we state this with pain, as we are aware that its doctrines have been upheld and defended by, at least, one

learned and most conscientious member of this Association. But, in making this assertion, we impute no shade of deceit or imposture to many excellent and single-minded individuals—far from us be the thought thereof; but we do believe them to have laboured under false impressions, and that the mesmerised themselves have often been thoroughly convinced that they were under the influence of some extraordinary power. We ourselves adhere to the opinion of the great majority of the profession,—that all that has appeared wonderful in mesmerism, exclusive of manifest imposture, may be explained by natural causes and effects, without the aid of any specific power hitherto unknown or unappreciated.

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Works of transcendent merit are in our science comparatively scarce, for genius alone, without a long and patient train of study and observation, will not, either in the practical or literary department of medicine, ensure permanent success. We have already noticed in the course of our observations, several of the more important books that have appeared during the last twelve months, but the bare catalogue of all that has been published would extend far beyond our prescribed limits. Physiology has been enriched greatly by the anatomical and pathological observations of the Messrs. Goodsir, by the essay of Mr. Simon on the thymus gland, and by the microscopical researches of many British and continental observers, among whose names we mention with great pleasure that of Mr. Addison, of Malvern. The works of Mulder, of Utrecht, have already been translated into several languages, and have obtained for the author an European reputation.

The pathology of cerebral disease has been illustrated by the works of Seipion Pinel, and of Rollet, of Nancy, and by the essays of Dr. Gustav Spiess, of Frankfort, and of Ludwig Mauthner, in Vienna. The work of Fourcault, on chronic diseases, comprises very valuable data regarding the origin and causes of pulmonary phthisis, and the lectures of Dr. Evans, of Dublin, on this malady, contain views differing materially from those generally received. Dr. Latham has republished his Lectures on “Clinical Medicine,” or, rather, has produced an entirely new work under this title, the first volume being devoted to the consideration of diseases of the heart; it exhibits all the originality and well-known talent

of this distinguished writer. Finally, we notice with peculiar satisfaction Dr. Golding Bird's excellent little volume on "Urinary Deposits," in which this difficult subject is treated with so much conciseness and discrimination, as to render the work a most valuable acquisition, both to the student and to the practitioner.

The difficulty experienced by those largely employed in the duties of our profession, and especially by that most important class, the general practitioner, of obtaining a good knowledge of the current medical literature of the day, will, we think, be greatly diminished by the publication of a periodical volume by Dr. Ranking, of Bury St. Edmunds, containing a brief but excellent summary of the most recent researches. The success of *Kleinert's Repertorium* and of *Schmidt's Jahrbücher* in Germany, and of Mr. Braithwaite's excellent Retrospect in this country, amply demonstrates the practicability of such an undertaking. Dr. Ranking has, in his first half-yearly volume, now before the public, adopted the excellent plan of engaging men eminent in various departments of our science, to furnish reports of the recent progress of those branches to which they have peculiarly devoted themselves.

### OBITUARY.

It is every year a mournful task to record the departure from this life of some of the brightest ornaments of our profession; but there is consolation in the thought that the memory of many of those who have gone before us will live, not only in the works that they have written upon science, but in the records of the good they have effected as members of the great Christian community. The stigma of infidelity, of unbelief in a high Providence and a merciful Redeemer, which has been so often and so unjustly imputed to medical men, cannot, at least, be laid to the charge of those whose death we have this year to deplore.

One of the brightest ornaments of the Christian world, as a moralist and as a religious-minded man, was Dr. Abercrombie, of Edinburgh, whose sudden demise we have already alluded to in another part of this Address. It is unnecessary to name his works; perhaps, of all others, his essay on the "Diseases of the Brain," was that on which his fame as a scientific pathologist principally rested, while as a Christian philosopher we may point with great pride to his "Essays on the Intellectual Powers," to his "Philosophy

of the Moral Feelings," and to his "Elements of Sacred Truth for the Young." We cannot pay a better tribute to the memory of so eminent a man, than by quoting from the last work the words of the eminent author in the preface:—"The ambition that now remains to him is to have his name associated with those solemn and sacred hours, when the Christian parent calls around him the children of his heart, and feeling all the uncertainty of life which is passing over them, seeks to raise their minds to a life that is never to end." And who will say that this holy ambition, even long before the author's death, was not amply realized?

In England Dr. W. Wright, of Norwich, Dr. Elliott, of Carlisle, Mr. W. Duke, of Hastings, Dr. James Thompson, of Burnley, and Mr. Kipling, of Newport Pagnel, have paid the great debt of nature. All these were men more or less distinguished in their respective spheres, though perhaps less generally known than Dr. Ingleby, of Birmingham, whose demise we must lament as a most serious loss, especially in that department of our science to which he had so prominently devoted himself. Edinburgh has lost also Dr. James Home, the late Professor of the Practice of Physic in that university, where, for a long period of years, he held different professional chairs. On the continent death has been less busy among the eminent men of our profession; but Paris must lament the demise of Breschêt, the industrious and distinguished cultivator of anatomy and physiology, and whose researches upon the skin have ensured for him a lasting reputation.

It has been to us a deep source of gratification, amid the heavy labour of condensing into this brief survey the enormous mass of materials presented to us during the past year, to know that we were to address an auditory well calculated to judge of our merits such as they might be, and disposed to overlook our deficiencies such as they really are. Whatever may be the defects of this Address, we trust our good-will in endeavouring to meet your wishes, will obtain your approbation.

30

ON THE  
MINUTE STRUCTURE OF THE LUNGS,  
AND ON THE  
FORMATION OF PULMONARY TUBERCLE.

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COMMUNICATED BY RICHARD D. GRAINGER, Esq.

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THE lungs are made up of bronchial tubes, bronchial intercellular passages, and air-cells.

*On the bronchial tubes and intercellular passages.*  
—The bronchial tubes commence at the bifurcation of the trachea. They are composed of cartilaginous rings and a proper membrane. They ramify in the substance of the lungs, their cartilaginous rings gradually disappearing; and in the human lung, having arrived within about one-eighth of an inch of its surface, the membrane also terminates, but somewhat abruptly,\* after which the passages conducting the air continue in the same direction as the bronchial tubes, of which they are the continuation, but without having any perceptible mem-

\* See Plate V., fig. 1.

branous lining; their parietes being formed merely by the air-cells between which they pass and by which they are surrounded.

The membrane of the bronchial tubes retains its fibrous character as far as its termination, the fibres being arranged longitudinally and circularly, and also its lining membrane. These are supplied by a distinct set of blood-vessels, which at the termination of the membrane anastomose with the vessels of the air-cells. The diameter of the ultimate bronchial tubes is from  $\frac{1}{50}$  to  $\frac{1}{30}$  of an inch. They communicate with but few air-cells, and at these communications their membranous lining is not continued into these cells, but, on the contrary, the vessels of the cells pass into the bronchial tubes, and ramify very superficially on their inner surface, probably to allow the blood within them to be acted upon by the inspired air.

The bronchial intercellular passages are at first of a circular form, and, like the bronchial tubes, do not communicate with many air-cells; but as they approach the surface of a lobule, the number keeps increasing, and at length these openings of communication are so numerous, and so near together, that the intercellular passage loses altogether its circular figure, and becomes reduced to an irregularly-shaped passage, running between the air-cells, and communicating with them in all directions; lastly, having arrived close to the surface of a lobule, it terminates in an air-cell, which is not dilated, as stated by Reisseissen, but has about the same

diameter as the passage of which it is the continuation.\* The epithelium (ciliated) which is peculiar to the air-passage, lines the bronchial tubes as far as the termination of the bronchial membrane, but it does not appear to extend beyond the membrane into the bronchial intercellular passages, or into the air-cells. The smallest tube in which I have seen the ciliated epithelium, had a diameter of about  $\frac{1}{30}$ th of an inch, but I have never found it in a tube where the bronchial membrane was not present also. As the lung near its surface is made up entirely of air-cells and intercellular passages, the contents of these can easily be examined, without there being any admixture of the epithelial lining of the bronchial tubes, care being taken only to select for examination those portions of the lung which lie immediately beneath the pleura. I have repeatedly examined these parts of the lung, but have not found this kind of epithelium. In the healthy lung, black pulmonary matter is present in greater or less abundance in the air-cells, as well as in the cellular tissue, distending the interlobular fissures and adhering to the external surface of the blood-vessels ; and, in the diseased lung, granular matter may sometimes be found in the intercellular passages and air-cells, but it has not the character of ciliated epithelium.

In the uninjected lung, the remains of nucleated cells in the walls of the capillaries have a sufficient

\* See Plate V., fig. 2.

resemblance to epithelium to be easily mistaken for it. Mr. Addison seems, from the following observation, to have fallen into this error :—" They (the air-cells) possess an epithelium, in the form of round nucleated scales, and from one to fifteen or more nuclei may be counted in a single scale." Mr. Addison, however, adds,— " But I have never satisfied myself that they possess the ciliated cylinder epithelium, so abundant in the trachea and bronchi."\*

From what has been stated, it appears that the upper part only of the air-passages is lined by mucous membrane, and it will be shown hereafter that those parts in which the aeration of the blood more particularly takes place, are lined only by a very thin fibrous membrane. These facts seem to agree with the phenomena presented by acute inflammation of these structures, inflammation of the bronchial membrane (bronchitis) being attended by the symptoms peculiar to inflammations of other mucous membranes, and inflammation of the membrane lining the air-cells (pneumonia) being accompanied by deposition of fibrine, as in inflammation of the common fibro-cellular tissue.

The existence of a difference in structure between the membrane lining the bronchi and that lining the air-cells was long ago inferred, and insisted upon, by Dr. Addison and others, from pathological considerations ; but, I believe, the fact has not been *demonstrated*.

\* Philosophical Transactions, 1842, Part II., p. 162.

*On the air-cells.*—The air-cells are small, irregularly-shapen, and, most frequently, four-sided cavities, varying in size in different parts of the same lung, those being the smallest, as well as the most vascular, which are situated nearest its centre, whilst their size gradually increases, and their vascularity diminishes, as they extend into the more remote parts. This difference in size and vascularity of the air-cells is probably for the purpose of adapting the quantity of blood requiring to be decarbonized, to the deteriorated condition of the air in the differently situated cells. The renovation of the contents of the more superficial cells taking place more slowly than that of the contents of the more central ones—it being in the former effected by the passage of the air from the terminal into the more central cells (in consequence of the law of diffusion of gases), and in the latter, chiefly by the mechanical dilatation and contraction of the thorax,—it is necessary that there should be a proportionally small quantity of blood circulating through the former, than through the latter.

The air-cells which are situated close to the bronchial tubes, or intercellular passages, open into them by large circular apertures, whilst those which are placed further from these passages communicate with them through the medium of other cells. These communications of one cell with another are of the same shape and size as those which exist between the first set of cells and the bronchial tubes; and they can be seen very distinctly by looking into

the air-cells from the intercellular passages, and regulating the distance of the object-glass according to their different depths. As these openings are not necessarily in a straight line, the exact quantity of cells which communicate cannot in this manner be determined, but the number will depend upon the distance which intervenes between any given part of a bronchial passage and the surface of a lobule; so that, when a bronchial passage arrives nearest the surface, it will be separated from it only by a terminal cell, as before observed.

Besides these intervening air-cells, there are others which fill up the angle formed by the bifurcation of the intercellular passage, and which thus appear to form a cellular communication between them.

It is very easy, in an injected preparation, to see the communication between two, or sometimes three cells; but, to determine with exactness the number of cells which communicate in succession, is probably impossible, in consequence of the section which is favourable for displaying the opening of one, being unfavourable for displaying that of the other; but, that the communications between the cells of the same lobule are very free, is obvious from the fact, that, if injection be thrown into a small bronchial tube, it will distend all the cells of that lobule, while none of the injection will pass into the adjoining lobules. In the mammal, the walls or partitions by which the air-cells are separated from one another, consist of a single plexus of vessels enclosed in a fold of membrane, whilst the

sacculi of the lung of the reptile, which may be considered to correspond to the air-cells in the lung of the mammal, are separated by a plexus folded on itself, and, therefore, consisting of two layers of vessels, a character by which the lung of the reptile differs from that of the mammal.

The plexuses in the lungs of the reptile and mammal consist alike of a very dense net-work of capillaries, into which terminal branches of the pulmonary arteries terminate, and from which the radicles of the pulmonary veins take their origin.

In the latter—the mammal—the number of capillary plexuses is not, as some have supposed, the same as that of the air-cells, that is to say, a terminal artery does not divide into a plexus at any particular part of a cell, its branches uniting for the commencement of a vein on the opposite part. On the contrary, one plexus passes between, and supplies several cells. In the interior of the lung, the exact extent of an individual plexus cannot be determined, in consequence of the removal of some part of it by the section necessary for its exhibition. But, on the surface of the lung where the extent of these plexuses, in relation to the cells over which they ramify, can be easily made out, an individual plexus may be seen to spread over an area of ten or twelve cells in some parts, and fewer in others, the exact number depending, in some measure, upon the size of the cells.

Here, after the pleura has been removed, the terminal arteries may be observed descending from the

interior of the lung between the cells to the surface, where they send off their branches in all directions, some anastomosing with the adjacent branches of the pulmonary artery, and others terminating in the radicles of the pulmonary veins. |

On that side of a cell in which is situated the opening of communication with an adjoining one, the capillaries anastomose all around this opening, so as to appear in the injected preparation, by reflected light, to form its immediate boundary; but by transmitted light, this opening will be seen to be formed by circular threads of the lining membrane of the air-cells, which extend beyond the circle of vessels.\* To these communications, Mr. Addison has given the name of "lobular passages," under the idea that no air-cells exist in the foetal lung, but that they are formed after birth by the mechanical dilatation of the bronchi.

The incorrectness of this notion is obvious from this fact, that the air-cells, with the capillary plexuses between them, are perfectly developed in the lungs of animals which have died two or three days after birth; for, in such cases, it cannot be supposed that these vessels could have been formed in so short a time as that which had elapsed between the birth and death of the animal, even had the parietes of the bronchi been dilated in the manner supposed by Mr. Addison.

With a view of settling this question, at the suggestion of Mr. Grainger, I injected the lungs of va-

\* See Plate V., fig. 3.

rious foetal animals, which had never breathed, and found, upon examining them with the microscope, that the air-cells were developed proportionally with the other parts of the lungs.

In the very young foetus, the septa between the air-cells consists almost entirely of minute cellules or granules, and a small quantity of fibrous tissue, with scarcely any blood-vessels, so that the injection, which had been thrown into the pulmonary artery and extended into these vessels, became extravasated among the granules situated between the air-cells.

As the age of the foetus advances, the granular matter diminishes, and the capillaries increase, so that at birth, the same arrangement of the air-cells and the other parts of the lungs exists as in after-life.

It has already been stated that the capillaries of the lungs are situated in a fold of membrane.\* Proceeding with a description of this membrane, from the peripheral to the central parts of these organs, it may be described as lining, first the air-cells, which are next to the surface of a lobule, that is, those which are situated immediately beneath the pleura, and those which bound the interlobular fissures, being separated merely by the capillaries from the pleura in the one situation, and by the capillaries from the interlobular areolar tissue in the other.

From these cells it passes into the next set, enclos-

\* See Plate V., fig. 3.

ing the capillaries, which are situated between them, and forming the immediate boundary of the opening by which they communicate: thence extending from one cell to another, it arrives at the intercellular passages, and, at the termination of the bronchial tubes, becomes confounded with the bronchial membrane.

This membrane is so thin and transparent that it can be seen only by transmitted light; it is distinctly fibrous; its fibres are strongest and best marked the nearer they are situated to the openings of communication between the air-cells, and some circular fibres extending a little beyond the vascular anastomoses which surround those openings, form their well-defined border.\* This membrane besides lining the air-cells, and supporting the capillary plexus, will serve especially by means of its circular fibres to keep the openings into the cells patent. Its structure has no resemblance whatever to muscular fibre, either of the striped or unstriped kind.

*On the formation of pulmonary tubercle.*—Having now considered the anatomy of the lungs, I will proceed to speak of the formation of pulmonary tubercle, and to show the manner in which the tuberculous deposit takes place, and extends itself by the destruction of the various parts of the lungs.

In order to determine with accuracy the precise situation of tuberculous matter, and to observe the manner in which the lungs become progressively destroyed by its presence, it is advisable to examine

\* See Plate V., fig. 3.

tuberculous lungs which have been successfully injected.\*

In such preparations, when viewed by reflected light, the pale colour of tuberculous matter contrasts so strikingly with the red colour of the injection in the capillaries of the walls of the cells, that there is no difficulty in discerning its exact form and limit, even in quantities so small as to fill only one, or even a small portion of a cell.

By this mode of examination, it will be clearly seen that the tuberculous matter is poured from the free surface of the pulmonary membrane into the interior of the air-cells. These becoming distended, and the septa between the contiguous cells being at first compressed, and their vessels afterwards obliterated, the supply of blood to the diseased part is cut off, and a tubercle formed, corresponding in size to the number of distended cells.

The manner in which tubercles extend themselves to the parts adjacent is best seen by observing the progress which had been made by the deposition in the air-cells situated in their vicinity. Here some of these cells will be seen to contain only a small quantity of deposit, others to be completely distended with it, though their vascular walls remain entire. Between other cells containing tuberculous matter, these vessels will be seen to have become partially obliterated ; and lastly, in the septa, between those cells where the accumulation has amounted to such a quantity as, generally, to com-

\* See Plate V., fig. 4.

press the capillaries, and probably cause their absorption, no vessels are left, and nothing exists but the pulmonary membrane, which remains mixed with the tubercular matter, and which (the tubercle having been broken up) is in a state to be ejected from the lungs in the expectoration, in which it can be detected by the microscope.

An imperfect examination of the vascular connection of a tubercle with the surrounding air-cells, in the injected lung, might give the idea of vessels passing into it from the adjacent unaffected parts, and thus lead to the supposition that it is vascular,—an idea which has long been prevalent among pathologists. A careful examination, however, clearly shows that these vessels are merely portions of plexuses which have not yet become absorbed. In most cases, these vessels will appear as arcs of circles of greater or less extent, of the same radius as the plexuses between the air-cells, of which they are, obviously, the remains.

The deposit of tuberculous matter takes place in the bronchial intercellular passages at the same time and in the same way as in the air-cells; and their walls,—which are in reality the air-cells between which they pass,—disappear in the same manner as those which separate one cell from another; and thus these passages being occupied by tuberculous matter, contribute to form a part of the tubercle. The smaller bronchial tubes also becoming distended with tuberculous matter, are involved in the general mass.

It will be obvious from what has preceded, that as a tubercle increases in size, the central parts of it will become further and further removed from those vessels, by which the tuberculous matter was in the first instance deposited, and afterwards maintained in a state of vitality, and consequently these parts will have the greatest tendency to lose their cell or vegetable life, and become softened. According to this explanation, this process ought always to begin in the centre of a tubercle, and I believe this is generally the case. But it must be recollected that the geometrical centre of the mass is not necessarily the point furthest removed from the source of circulation ; the point may even be on the side of the tubercle, provided it be the remote cells of a lobule which are occupied by the tuberculous matter, namely, those bounded by an interlobular fissure : in such a case, this part of the tubercle might be further removed from the source of circulation, than its exact centre.

Some pathologists contend that tubercle is the result of inflammation. Without entering at length upon the consideration of this question, which is one *de verbo* and not *de re*, I may state that the perfectly natural appearance of the vessels close to a tubercle, and even of the cells containing a small quantity of tubercular matter, not sufficient to have impeded their circulation in the capillaries during life, when compared with the tortuous and unequally dilated state of vessels going to air-cells filled with fibrine, in consequence of inflammation, are patho-

logical considerations in favour of the non-inflammatory nature of phthisis. These facts go also to show that the obliteration of the capillaries between the air-cells is, in phthisis, produced by some force which has exerted upon them a slow and very gradual compression, such as can only be conceived to have been produced by the accumulation of tuberculous matter, in contiguous cells, pressing upon the intervening plexuses.

I may observe, that the facts I have mentioned refer only to the most ordinary description of phthisis, and that they are the result of a great number of examinations; and I contend that they prove that this form is not at all connected with other forms, and that it is erroneous to suppose that miliary is necessarily the incipient state of common tubercle. My preparations, as well as those of Mr. Quekett, show that the progress of this tubercle, from its commencement up to its perfect formation, depends, simply, upon the quantity of the deposit; for it can be seen, in some parts, occupying only a part of a cell, in others, one, two, three, or even of an indefinite number, and, in every case, to exhibit the same microscopic characters.

In this form of phthisis there appear to be two pathological states from which we can deduce an explanation of its symptoms:—first, a portion of lung is rendered impermeable to the blood; secondly, the blood is thrown upon the surrounding unaffected parts.

Whether the first of these states ought to be regarded as inflammation, or not, must ultimately remain a matter of opinion, so long as the term "*inflammation*" has no definite signification.

Without doubt, it is to the second state as a mere consequence of the first, that the attacks of haemoptysis are due, as well as the susceptibility to severe and obstinate catarrhal symptoms. The tuberculous matter, acting like an extraneous body, will produce a constant tendency to inflammation, and, after it has been produced, prevent its removal. Hence will follow those hectic and other symptoms which result from the continued operation of any irritating body.

As regards the expectoration, this will occur most frequently from the bronchial membrane, and, most probably, is not to be distinguished from that in ordinary bronchitis. It will be only during the breaking up of a tubercle that matter truly tuberculous will be expectorated, and this, I believe, can be recognized, with certainty, by no other character than its containing fragments of the membrane of the air-cells.

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P.S.—Since this paper was communicated to the Society, I have met with an instance in which tubercles existed in the lungs, liver, kidney, mesentery, and other parts,—all evidently of a scrofulous character. I injected the animal (a rabbit) with fine injection. Some parts of the lungs were studded with white masses of different sizes; others,

even as much as the third of a lobe, appeared very much like a lung which had never respired. On examining the latter, I perceived in the arterial trunks leading to those parts, distinct masses of white granular matter mixed with the injection; and, continuing the examination, I found that this appearance was due to all the capillaries being, literally, choked up with this same matter. The air-cells were free from it, and contained air. The white masses in the other parts appeared to be produced by the vessels being filled with this matter, as in the preceding, and also by its escape into the air-cells and surrounding structures. On examining the kidney, I found that the vessels were filled in the same manner as in the lungs. I mentioned this to Mr. Quekett, who told me that he had, in scrofulous cases, seen strumous matter mixed with blood, which had been pressed out from an artery going to a diseased part.

[*From Transactions of Medico-Chirurgical Society, Vol. xxviii.*]

## EXPLANATION OF THE PLATE.

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Fig. 1.—Shows the abrupt termination of a bronchial tube in a bronchial intercellular passage.

- a* The bronchial membrane.
- b* The opening of an air-cell into a bronchial tube, with the capillaries surrounding it.

Fig. 2.—The course and termination of a bronchial intercellular passage.

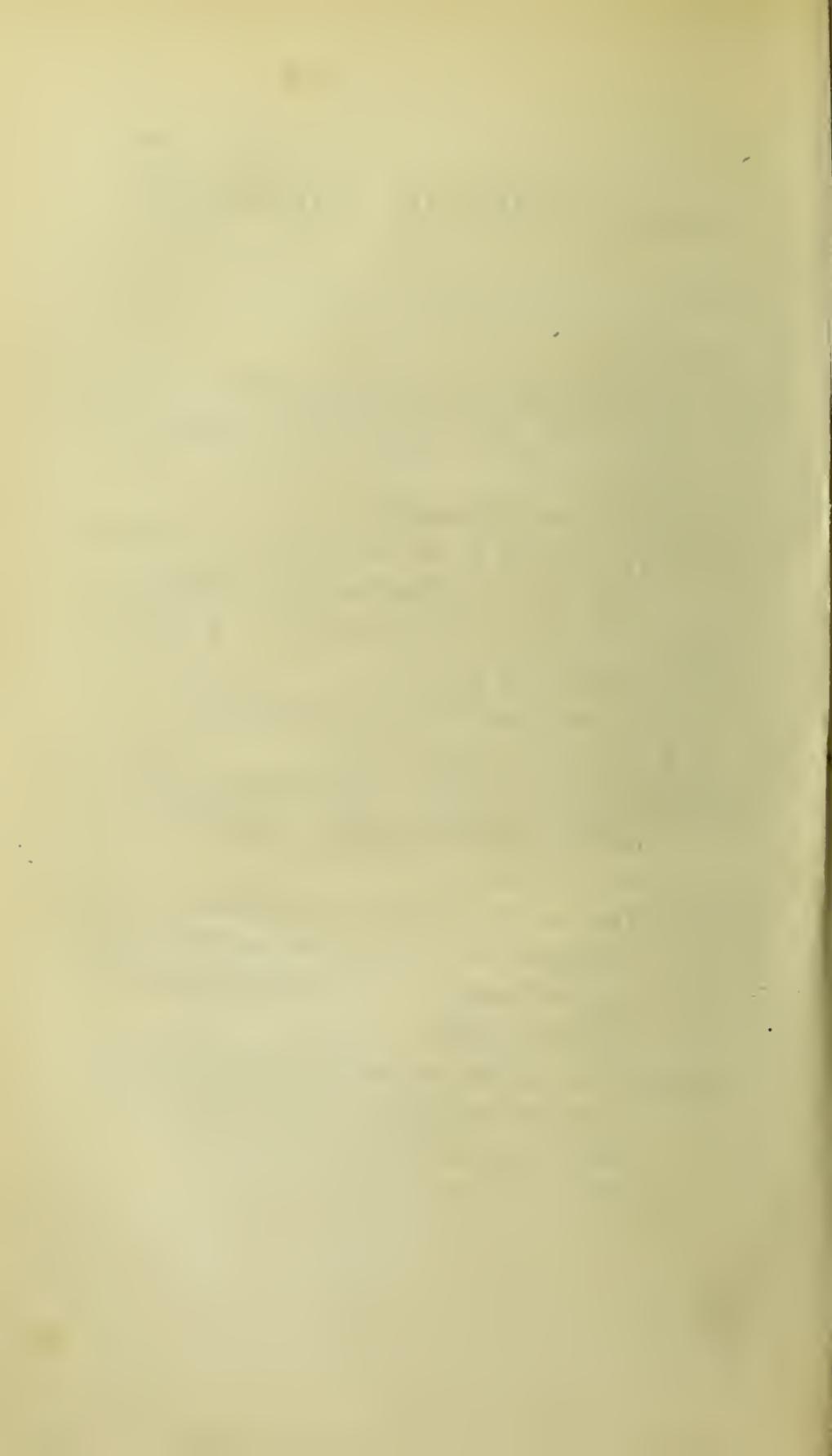
- aa* Its commencement and termination.
- b* An air-cell opening into it.

Fig. 3.—The pulmonary membrane enclosing the capillary plexuses highly magnified.

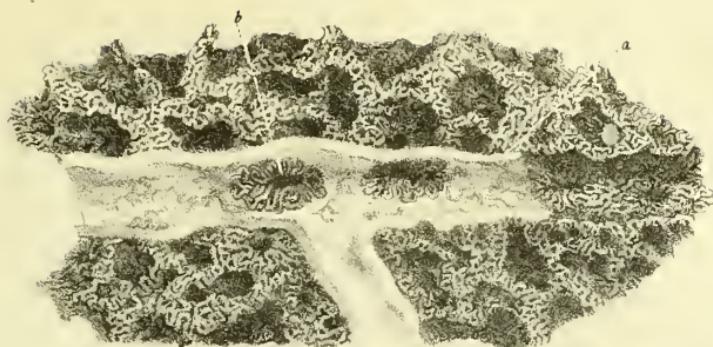
- a* The plexuses.
- b* The free margin of the membrane, projecting beyond the vessels, and forming the immediate boundary of the opening of one air-cell into another.

Fig. 4.—The tubercular matter situated in the air-cells with the remains of the capillary plexuses between the cells.

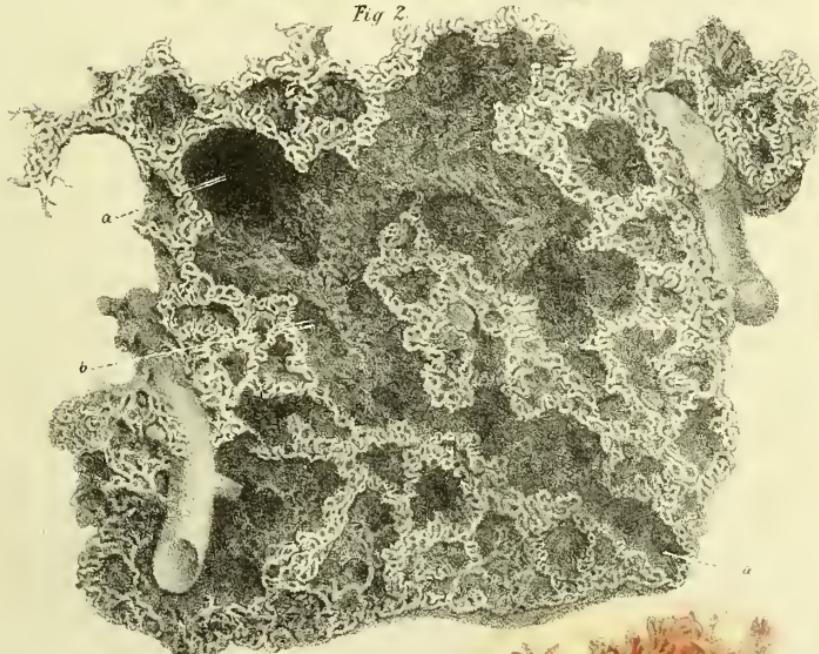
- a* The tubercular matter.



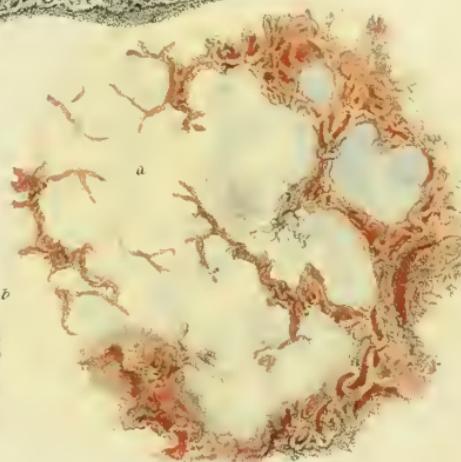
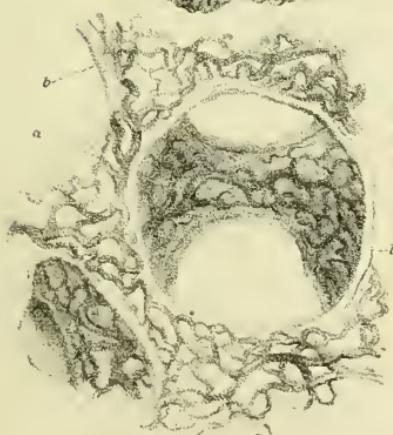
*Fig 1.*



*Fig 2.*



*Fig 3*



*Fig 4*



XLIII.—*Anatomical and Physiological Observations on some Zoophytes.* By JOHN REID, M.D., F.R.C.P.E., and Chando Professor of Anatomy and Medicine in the University of St. Andrews.

[With a Plate.]

In the following observations upon the structures and actions of some of the Zoophytes obtained from the shore of the bay of St. Andrews, I have confined myself to those points which are either new, or which appeared deserving of additional illustration. In using the terms *superior* and *inferior*, *upper* and *lower* in reference to the *relative* position of different parts of the polypidom, in the descriptive parts of this paper, the polypidom is supposed to be in the erect position, so that these terms correspond to *anterior* and *posterior* when the polypidom is placed horizontally. In using the term *anterior surface*, I mean the surface on which the apertures of the polype-cells are placed, so that this corresponds to the upper surface when the polypidom is laid horizontally for examination.

*Cellularia reptans.* This polype grows in considerable abundance close upon low-water mark, on the exposed surface of a stratum of clay-slate and conglomerate, interposed among strata of sandstone belonging to the carboniferous series. Growing along with it, but in much smaller quantities, are *Cellularia scruposa*, *Crisia chelata*, *C. eburnea*, *Pedicellina echinata*, *Vesicularia spinosa*, *Valkeria imbricata* and *Plumularia falcata*, none of which have I hitherto found adhering to the surrounding strata of sandstone.

The polypidom of this polype possesses some structures which as far as I am aware have not yet been described. At the external and upper angle of the cell, and posterior to the two spines attached to this angle (Pl. XII. fig. 1 *a*, fig. 2 *c, a, b*), three of these structures are found\*. The uppermost of these is a hollow process (fig. 2 *b*), the superior extremity of which is free, looks outwards and a little forwards, and has an aperture notched on the

\* Part of this process is seen on looking at the anterior surface of the polypidom, as is represented in Plate XII. fig. 3 *b*.

lower and upper edges, but more deeply in the former than the latter. From this aperture a hair-like prolongation (fig. 2 *d*), about the length of the cell, and slightly curved, projects. The interior of the process is filled with a fibrous contractile substance which moves this hair-like prolongation. Its movements occur at irregular, occasionally very short intervals, and it sweeps downwards over all the posterior surface of the polypidom within its reach. It then turns back upon its former track, ascending upwards until it reaches again the outer edge of that part of the polypidom lying above the process to which it is attached; it now descends in the opposite direction over the outer part of the polypidom, and places itself along the outer edge of that portion of the polypidom lying below it. From this it re-ascends in the course just described. The extent of these movements is increased by the presence of the notches in the edges of the aperture through which the hair-like prolongation passes. These movements are perfectly independent of the polype and continue for days after its death. The upper and outer edge of the polype-cell is prolonged into a process (fig. 1 *a*, and fig. 2 *c*) mucronated at its external and upper angle. This process is hollow and is filled with a pale fibrous contractile substance, which I have frequently seen become elongated and rise in the form of a short conical eminence above the upper edge of the process, and then after a while it contracted suddenly and retired within the process. This process was in some cells metamorphosed into a strong spine (fig. 1 *b*), and in such cases three spines were attached to the external angle of the cell instead of two the normal number. It has an affinity with the tooth-like process of *Cellularia scruposa*, as both contain a similar contractile substance. Placed between the bases of the above two processes and overlapping the latter, is a rounded small cavity with a distinct circular aperture (Pl. XII. fig. 2 *a*). In some cells all these three appendices are wanting; in others only one of the two former is present. The polype protrudes itself through a small aperture directed outwards and upwards, placed at the upper end of the cell and towards its outer edge, and immediately in front of the process bearing the hair-like prolongation (fig. 3 *a*). This aperture is crossed anteriorly by a pretty strong rim which forms the upper edge of the anterior surface of the cell, and posteriorly by the still stronger rim forming the upper edge of the posterior surface of the cell. Below this aperture there is a considerable portion of the anterior wall of the cell formed by a transparent membrane, and bounded by a thick edge, constituting the large oval opening in the anterior wall of the cell in dead or dried specimens. In the greater number of cells this space is crossed by bars of calcareous matter, growing from its inner margin by one stem which generally divides dichotomously into

four, and these increasing in length reach its outer margin (fig. 3 *a*). These bars are hollow, are lined internally by a fine membrane, and almost entirely disappear when the polypidom is immersed in dilute muriatic acid. Neither these bars nor the three appendices to the cells above described, present themselves until the body of the cell and its containing polype have been fully formed. The spines attached to the cell are almost always four in number,—two to each superior angle of the cell,—are hollow, and the external two are longer and stronger than the internal. The two former are of considerable thickness, and are generally as long, sometimes more than twice as long, as the cell.

The polype has from fourteen to sixteen ciliated tentacula, of a light orange-colour, rather more than three-fourths of the length of the cell. The animal when retracted within its cell is folded up as in *Flustra foliacea*. Fig. 5 is a representation of the polype when expanded, and fig. 4 represents its appearance as seen from the posterior surface when it withdraws and folds itself within the cell. In this polype the part marked *a* in the figures had more of the appearances of an appendix of the stomach (*b*), or of a separate organ, than in some of the other ascidian polypes\*. Its inner surface is so thickly covered with reddish brown granules, or more properly speaking, minute cells, that it is quite opaque. Similar granules also adhered to the inner surface of the œsophagus (*d*) and stomach, and sometimes in greater number to the former than the latter. The inner surface of the pharynx (*f*), the œsophagus, the stomach, and a portion of the intestine (*c*) next the stomach are covered with cilia. A mass of dark-coloured egesta, apparently principally composed of the cells and granules thrown off from the inner surface of the digestive tube, is frequently observed about or above the middle of the intestine, and this part of the intestinal tube presents a dilatation frequently considerably larger than what is necessary to contain the inclosed mass. The polype in protruding itself first pushes out a short flexible tube attached to the inner margin of the aperture through which the tentacula pass. The muscles by which it withdraws itself within its cell are two in number,—one proceeding from the lower and outer part of the cell, and dividing into two bundles as it passes upwards, which are attached to the sides of the lower part of the pharynx; the other arising from the lower part of the cell and attached to the lower end of the appendix of the stomach (fig. 5 *a*). The muscular bundles by which it protrudes itself cannot be distinctly traced from their proximity

\* From the contractility of these parts the form is not uniform, and in some individuals we find the stomach less and the appendix larger than they are here represented.

to the tentacula and intestine, but are seen passing downwards from the upper part of the cell along the sides of the tentacula to reach the gullet, and probably also the upper edge of the stomach. The flexible tube or operculum is retracted by two muscular bundles, one on each side, arising from the inner sides of and a little below the aperture of the polype-cell, and are inserted into the inner surface of the flexible tube. The young polype-cells, formed at the upper end of the branches, grow from the posterior surface of the polype-cells last formed a little below their upper margin. Their first appearance is that of a rough transverse line occupying the inner portion of that surface. Several specimens presented the bodies frequently termed opercula, but which we shall call *ovary-capsules*, placed as usual at the upper end of the polype-cells, and were here somewhat nearer their inner than their outer margins. The contents of these we shall describe in a subsequent part of this communication.

*Cellularia scruposa*. This polype is found, as I have already mentioned, in the same locality with *C. reptans*, and it is also thrown ashore from deep water, sometimes in considerable quantities and of more luxuriant growth, chiefly adhering to *Flustra foliacea* and *F. truncata*. A perpendicular hollow process springs from the upper and outer edge of the cell immediately above the already well-known tooth-like process, and adheres to the lower part of the outer edge of the cell immediately above (fig. 6 a, and fig. 7 b). The aperture of this process is pretty deeply notched before and behind, and its interior is filled with a contractile fibrous substance which moves a curved hair-like prolongation (fig. 7 b) about the length of the cell, which sweeps at intervals over both the anterior and posterior surfaces of the polypidom within its reach. It rises up slowly over the anterior surface, makes a sudden jerk over the outer edge of the polypidom, and proceeds slowly downwards over the posterior surface as far as the notch in the aperture permits, and after remaining at rest for a longer or shorter time, it returns along the same course to the position from which it started. In this movement it performs a slight rotatory motion, so that its concavity is always directed towards the surface of the polypidom. This hair-like prolongation, in this as in *Cellularia reptans*, tapers gradually towards its free extremity, and is not rounded but flattened. In the *C. reptans* I never observed this hair-like prolongation cross the anterior surface of the polypidom, except when placed at the angle of the bifurcation of a branch. The use of these hair-like prolongations may probably be to keep the surface of the polypidom clear of substances which would otherwise adhere to it. Their motions are executed with more force than we should at first suspect. I have seen one of them in its course encounter the

stalk of a *Pedicellina echinata*, and press it aside. The tooth-like process (fig. 7 c) is hollow, has an aperture in its upper edge, and in several specimens I have observed it filled with a fibrous contractile substance which expands and rises upwards through the aperture, and after remaining stationary for a time it re-enters the process. It rises only a short distance above the aperture, and when expanded presents the appearance of the upper and outer angle of the containing process with the curve turned in the opposite direction. When expanding it moves from without inwards, gradually rising above the edge of the aperture, and it re-enters the process by a sudden jerk in the opposite direction. These movements of expansion and contraction commonly occur after long intervals, and it is in general only by watching a portion of the polypidom for a considerable time under the microscope that they can be detected. More rarely these movements occur in rapid succession. I can form no conjecture regarding the function of this curious contractile substance. At the root of the process bearing the hair-like prolongation there is a small rounded cavity with an aperture in its posterior wall, exactly like that described in the corresponding position in *C. reptans* (fig. 7 a). Each cell has four small hollow spines attached to its upper edge, two adhering to each angle. These spines are very considerably smaller than those in *C. reptans*, and in old specimens are generally broken off. The position of the aperture in the cell through which the polype protrudes is similar to that in *C. reptans*, and is also provided with a short flexible tube, which acts as an operculum when the polype retires within its cell. Many specimens are provided with ovary-capsules placed as in *C. reptans*. The polype has generally twelve tentacula of a light orange-colour, and has in other respects a great resemblance to that in *C. reptans*, and is provided with the same muscular bundles for effecting its movements and closing the operculum.

*Cellularia avicularis*. I lately obtained a large and very perfect specimen of this polype. The shape of the polype-cell, as Dr. Johnston remarks, is similar to that in *Flustra avicularis*. The bird-process is also exactly alike in both. It can, however, be readily distinguished from the latter by all the branches being composed of two rows of semi-alternate cells, and each cell having only two conical spines directed upwards or in the line of the long axis of the cells, and a little outwards and forwards, and attached to the angles of the superior margin of the cell. In a small number of cells an additional small spine, making three in all, projected from the outer angle in the same direction as the normal one. On the other hand, almost all the cells in *Flustra avicularis* have four spines, which differ in appearance from those of *Cellularia avicularis*. This specimen when dried assumed

only a very faint ash-colour, very different from the much deeper ash-colour in all the dried specimens of *Flustra avicularis* I have seen.

These two polypes ought certainly to be classed as two different species of the same genus, and not under two different genera. A new genus should perhaps be instituted for their reception, as their general character, and more especially the possession of those remarkable appendices, the bird-head processes, separate them from *Acamarchis*, *Flustra* and *Cellularia*, the genera to which they are most allied.

*Pedicellina echinata*. This polype is found in considerable quantities in front of the Castle of St. Andrew and near low-water mark, adhering to *Cellularia reptans*, to *Sertulariæ*, and to the surface of stones. It is more hardy than most of the other ascidian polypes, and can be kept alive at home for a long time. The number of tentacula varies from fourteen to twenty. In some specimens the stalk is nearly smooth, in others several spinous-looking processes project from it, and in others both stalk and body are covered with a long, fine and sparse down. In the young animal the body is relatively longer and narrower. The body in the older animal is very decidedly compressed from before backwards and elongated transversely, and is considerably narrower and more bulging at the edge in which the intestine lies (fig. 8 d) than at the edge next the gullet (fig. 8 a). The upper part of the body is bounded by a slender rim to which the tentacula are attached. This rim slopes slightly from the narrow towards the broad end of the body. The tentacula at the extremity of the narrow end are shorter than the others, and all of them become considerably broader as they approach the rim. They are connected together at their lower third by a contractile membrane, partly composed of circular fibres. The body itself is not contractile. The inner surface of the edges of the tentacula and the inner surface of the rim are provided with strong cilia, and in the older animals the external surface of the tentacula is frequently covered with a layer of pretty large granules or cells. On examining the animal under the microscope when placed in water containing a quantity of carmine, the movements of the currents of water produced by the cilia can be more distinctly observed. The two rows of cilia attached to each tentaculum do not produce currents in opposite directions, but both strike downwards and towards the mesial line of the tentaculum to which they are attached, and cause a current down the centre of its internal surface, by which the particles of carmine are carried downwards to the rim. When all the currents carried down the tentacula arrive at the rim, they are rapidly conveyed along its upper edge by the action of the cilia with which this portion of the inner surface is so abundantly

provided, towards the mouth (fig. 8 *a*). At this part all the currents converge, and thus produce an upward central current, by which the particles of carmine are carried outwards. None of the carmine, as far as I could observe, entered the oesophagus. The particles of carmine sometimes collected in considerable masses around the mouth before they were floated outwards. As the termination of the intestine opens near to the mouth, and at a point within the influence of this outward central current, the egesta when voided are rapidly carried away. It would thus appear that when substances not fitted for the nourishment of the animal are conveyed towards the mouth, the walls of this aperture are endowed with a specific property of irritability by which they are thrown into contraction and prevent its entrance. Such substances on the other hand as are capable of nourishing the animal do not act as excitants to this property of contractility, and they may be carried inwards. The possession of such a property is probably necessary for the existence of the animal. In this animal, as is well-known, the whole digestive tube and the ciliary motions on its inner surface can be distinctly seen through the transparent body. The walls of the stomach (fig. 8 *b*) and the first portion of the intestine (duodenum ?) (fig. 8 *c*) are very much thicker than the rest of the digestive tube, and were never observed to contract; and this last circumstance, viz. the non-contractility of these parts of the digestive tube, does not exist, as far as I am aware, in any other ascidian polype. A slight contractile movement was observed in a few cases at the upper part of the gullet. The last part of the intestine (fig. 8 *d*), which is not provided with cilia, contracts and expels the egesta which have previously accumulated there, frequently in considerable quantity. Brownish masses, apparently chiefly composed of the granules and cells which so abundantly line the inner surface of the stomach, are frequently seen in rapid rotatory motion in the stomach and duodenum.

The life of the body is of shorter duration than that of the stalk, and I have observed in several specimens the body fade and fall off, and a new one reproduced in its place. A few days before this takes place, the tentacula are permanently bent inwards and the membrane surrounding their lower part remains contracted, so as to completely, or nearly completely, cover the upper surface of the body, presenting in fact the appearance which the animal temporarily assumes when disturbed. The body then becomes more opaque and at last falls off. After this the stalk retains its property of alternately contracting and relaxing its different surfaces at intervals, upon which its movements depend. After the lapse of a few days the top of the stalk enlarges, and a minute head presents itself in which the different parts of the

body are developed. In the beginning of October I procured several specimens in which a large mass of cells (ova) was placed in the space between the gullet, intestine and upper edge of the stomach (fig. 8 *h*), extending downwards to the entrance of the gullet into the stomach, and depressing the stomach and forcing it considerably downwards. In two of these this mass of cells projected into the interior of the gullet near its lower part, and exceedingly minute ciliated ova were seen escaping from the upper part of the cellular mass, and several were also seen swimming in the interior of the gullet and stomach. Portions of this mass of cells were after a time extruded outwards, and were composed of the ciliated ova, and of very minute nucleated cells connected together by a structureless substance. Many of these ova formed a single cell, broader at one end than at the other, with a circle of cilia longer than the cell placed around the margin of the broad end (fig. 9 *a*), while others presented one, two or more very minute cells attached to its lower or narrow extremity (fig. 9 *b & c*). The nucleated cells consisted of a cell-membrane with two or more nuclei, and appeared to be undeveloped ova. The ciliated ova swam actively about, sometimes bending all their cilia in the same direction, forming a curved bundle and striking in the same line for some time together, at other times spreading their cilia and moving them in different directions. These ova are so minute as to require very high magnifying powers for their examination. It would thus appear that this polype, supposing all the individual animals whose stalks are attached to the same creeping stem to form one aggregate animal, extends and prolongs the life of the individuals composing it in two ways; viz. by renewal of the individual bodies after they have dropped off, and by offsets of new individuals from the creeping stem; and that it reproduces and extends the species, or forms new aggregate animals, by the formation of ciliated cells. I have never been able to detect any circulation of nutritious juices in the stalk, though examined under the most favourable circumstances.

*Crisia chelata*. This polype when extruded affords a good view of the membrane connecting the outer surface of the pharynx and rectum together (fig. 10 *a*). It would be more correct to say, connecting the *supporting part* of the tentacula and rectum together, for the pharynx, as in the other ascidian polypes, lies loose, and can be seen contracting, within this supporting part. It protrudes itself through a small opening at the upper margin of the cell, and the large opening seen in the dead specimen on the anterior surface of the cell, is in the living specimen covered in by a membrane. The polype has from ten to twelve ciliated tentacula about half the length of the cell. The dilatation of the digestive tube (stomach) at the termination of

the gullet and commencement of the intestine is smaller, and that part marked *a* in fig. 4 and 5 is relatively larger in this polype than in *Cellularia reptans* and *C. scruposa*, and has less the appearance of an appendix of the stomach\*. Its inner surface, however, is covered with a greater number of brownish granules than any other portion of the intestinal tube.

*Campanularia dumosa*. I have procured some live specimens of this polype thrown ashore after a storm attached to *Flustra foliacea*. The polypes and pith of the stalk are of a yellow colour. The polypes were sluggish, had twelve short tentacula not ciliated, and presented all the characters of the *Zoophyta hydroida*. Dr. Johnston writes me that he has also some time ago procured live specimens, so that he must be now aware that this polype cannot be a *Cornularia* as he once supposed (British Zoophytes, p. 192, 1838), and that the characters of the polypidom separate it from the genus *Campanularia*.

*Alcyonium parasiticum*. Abundance of this polype is occasionally thrown ashore chiefly adhering to *Sertularia argentea*. I have procured several specimens alive, and have satisfied myself that it consists of cells composed of animal and calcareous matter, and that the polype resembles the ascidian polypes in every respect. Mr. Hassall (Annals of Natural History, vol. vii. p. 370) first satisfactorily ascertained the true nature of this polype. On placing a portion of the polypidom under the microscope, and then bringing a quantity of dilute muriatic acid in contact with it, innumerable bubbles of gas are seen rising from all parts of its surface. On immersing another portion in aqua potassæ so as to destroy the animal matter, it lost its dirty brown colour, and the form and arrangement of the cells were then distinctly observed. Figure 11 is a magnified view of a few of the cells in the portion of the polypidom thus treated. Each cell is provided with a flexible tube attached to its margin, which the polype extrudes before it emerges from the interior of the cell, and retracts when it re-enters, thus serving the purpose of an operculum. The first portion of this operculum extruded, forms a small conical eminence with the apex truncated. When the polype withdraws itself within its cell, it frequently does not retract this portion of the operculum, so that the surface of the polypidom occasionally presents under the microscope a papillose appearance. The next stage in the protrusion of the polype is the elongation of this conical eminence by the eversion through it of a second portion, surmounted by pretty long setæ. The tenta-

\* As has already been stated, I have observed individual polypes both in *Cellularia reptans* and *scruposa*, but more especially the latter, where the difference between the size of the stomach and appendix was less marked than in figs. 4 and 5.

cula, by the upward motion of which the eversion of this flexible tube is effected, are now seen lying within it. The third stage in the protrusion of the polype is the passage of the tentacula and pharynx through the upper aperture of the flexible tube. The greater part of this tube appears to be composed of setæ connected together by a membrane. The polype has fifteen or sixteen tentacula. By breaking up a number of the cells I procured two of the polypes nearly entire, and the stomach and its appendix had nearly the same relative size as in *Crisia chelata*. Several bodies, each composed of reddish brown nucleated cells inclosed in a membrane (ova), were seen among the broken-down cells.

*Flustra avicularis*. This polype is thrown ashore in great quantities after storms, chiefly adhering to the roots of *Flustra foliacea* and *F. truncata*. The cells have almost always four hollow spines, adhering to the upper margin of the cell, two to each angle. The two superior spines are pretty long and project upwards and outwards, and the two inferior, which are placed close to the two superior at their origin, are considerably shorter and less thick, and project generally inwards, forwards and a little downwards. In a few cells I have seen five spines attached to the superior margin, three of these adhering to the outer angle. The bird-head processes attached to the outer edges of the branches of the polypidom are generally very considerably larger than those nearer their centres. Each bird-head process may be described as being composed of a *body* (fig. 12 *f*), of a *hinge-process* (fig. 12 *e*), and of a *pedicle* (fig. 12 *b*). By the pedicle it is attached to the interior of a round hollow process projecting slightly from the anterior surface of the polypidom (fig. 12 *a*). The body of the bird-head process\* is very convex along the lower edge, and it is elongated from below upwards and somewhat flattened transversely. It is divided by an oblique ridge on its interior surface into two chambers (fig. 12 *d*), which communicate freely at the superior and middle parts at least. The hinge-process is articulated to the superior or concave surface of the body by a hinge-joint, along the line of the superior termination of the internal ridge which divides the body into two parts. The edges of the concave surface are thickened at this part, and present a slight depression on each, for receiving the two articular processes of the hinge-process. The body of the bird-head process is hollow, and its concave surface presents three apertures; the largest of these is the uppermost, and is separated from the middle by a bar stretched across between the articular cavities

\* In describing this moveable bird-head process, I have supposed the polypidom to be erect, and the concave surface of this process to be looking upwards in the direction of the long axis of the cells.

for receiving the hinge-process; and the smallest is placed at the lower end, and affords a passage to the posterior part of the pedicle into the interior of the body. The hinge-process is concave on its upper surface, and terminates below in a curved point. Its superior wall forming its concave surface is deficient in two-thirds of its length at the upper part or that next the articulation, and its inferior or convex wall is very thin over the same extent. It is hollow, and communicates with the body through the upper and middle apertures seen in its concave or upper surface. Its upper or articulating end is bounded by a thickened portion or bar passing between the edges of the superior surface, and a similar bar passing between the edges of the inferior or convex surface. The articulating processes are placed upon the superior of these bars, at its junction with the edges of the superior surface. I have described, with what may appear very unnecessary minuteness, the skeleton of these bird-head processes, because it would be impossible to understand their movements without a previous knowledge of the different parts described. The lateral portions of the lower chamber of the body are occupied by two radiating muscles, presenting somewhat of the appearance of the temporal muscle in the human species, which converge at the articulating or upper edge of the hinge-process, and terminating in a denser, thicker and narrower structure, which I shall call tendons, are attached to and move this process (fig. 12 c). One of these muscles, which is the stronger, terminates in a tendon which runs above the transverse bar which separates the upper from the middle aperture in the concave surface, and running down the centre of the hinge-process is inserted into the inner surface of its inferior or convex wall a little above its apex or free extremity. When this muscle contracts, the hinge-process is tilted up. The other muscular bundle, which is strongest at the upper and lower edges, terminates in a tendon which passes beneath the bar, and is inserted into the hinge-process close to and a little above the tendon of the other muscle. When this muscle contracts, the hinge-process, if elevated, is drawn down. The first-described muscle is the *elevator*, the second is the *depressor* muscle of the hinge-process. The movements of the hinge-process are in general slight, but I have frequently observed it to be tilted up with considerable force, and closely applied over the superior surface of the anterior chamber, so that its concave, which was before its superior, became its inferior surface, and its convex became its superior surface. In this state it may remain for hours, and affords an excellent opportunity for observing the arrangement of the fibres of the two muscles, especially that of the elevator, as its lower fibres run more directly upwards, and its tendon is raised and separated from that of the depressor muscle. In dead specimens

the hinge-process is not unfrequently found in the position into which it is brought by the action of the elevator muscle. These muscular fibres present no transverse striae, can contract and relax with rapidity, and become shorter and thicker during their contraction. The movements of the body upon the polypidom are effected by the pedicle, and are as follows:—Suppose it to be attached to one of the edges of the polypidom, and the concave or upper surface to be looking upwards in the line of the long axes of the cells, it can turn slowly outwards over the edge of the polypidom until its concave surface looks directly outward, and it then returns to its former place: it may also turn inwards until the concave surface looks across the cells. This movement being suspended, it exhibits at intervals a nodding motion, the concave and convex surfaces being alternately depressed towards the anterior surface of the polypidom. When the concave surface is carried downwards, the hinge-process is slightly separated from the body; but when the convex surface is depressed, it is again approximated. These last movements of the hinge-process are probably in a great measure mechanical, and occasioned by it rubbing over the surface of the polypidom during the downward motion of the concave surface. The pedicle consists of two parts: a posterior and dense portion which is attached to the internal surface of the inferior edge of the process of the polypidom to which it is fixed, and passes inwards through the inferior aperture in the concave surface of the body to be inserted into the lower part of the internal surface of the convex surface of the body; and an anterior portion, more translucent and less dense, which is prolonged downwards into the process, and forwards to the middle aperture in the concave surface of the body and the attached end of the hinge-process. In the nodding movements when the convex surface is moved downwards, the posterior edge of the pedicle contracts and becomes bent so as to form an acute angle; and it relaxes while the concave surface of the body is moved downwards, resembling the contractile movements of the stalks in *Pedicellina echinata*. I have never had an opportunity of observing the changes in the pedicle during the other movements of the body under a high magnifying power, as this can only be done under certain conditions not easily to be obtained. The anterior portion of the pedicle has more of the appearance of a membranous than a contractile structure, and contains several small nucleated cells. A similar structure is found in the upper chamber of the body, and is prolonged through the upper aperture in the concave surface into the hinge-process. I have not been successful in observing contractile movements in this structure, if it really possesses this function, and I believe that it is more connected with the

nutrition of these bird-head processes than with their movements. It would be very interesting to ascertain the functions of these complex appendices to the polypidom. Their movements are quite independent of the polypes, and continue for days after these are dead. The hollow processes of the polypidom, at least those next the outer edges, to which these bird-head processes are attached, spring from the upper surface of canals which communicate with the interior of the spines, the ovary-capsules, and also by lateral apertures with the interior of the cells next them. Can these organs assist in circulating water along these canals ?\*

The body of the polype is very small when compared with the length of the cell, so that when it enters the cell, the gullet and intestine are not folded upon themselves as in *Cellularia reptans* and so many other of the ascidian polypes, but are simply thrown into a curve. It has fifteen or sixteen ciliated tentacula considerably longer than the body : the cilia are short, thick and numerous. In this polype, as in the *Crisia chelata* and *Alcyonidium parasiticum*, there is not so marked a division between the stomach and the part which has been termed the appendix, as in *Cellularia reptans* and *C. scruposa*. Brownish granules and minute cells are observed on the inner surface of the stomach, the gullet and commencement of intestine. Ciliary movements are distinctly seen in this as in the other ascidian polypes examined on the inner surface of the pharynx, gullet, stomach and first portion of intestine. In some specimens the polypes were very active, darting back into their cells when disturbed, and immediately after again protruding themselves. When left undisturbed, they at short intervals partially withdrew into their cells, and immediately after again emerged and spread out their tentacula. The movements of the cilia attached to the tentacula appear to be in this, as in other ascidian polypes, under the control of the animal. They remain quiescent when the tentacula are withdrawn within the cell ; and even when extruded their movements are occasionally for a time suspended. There can be no doubt that they can act also involuntarily, for they may be seen in full action upon detached portions of the tentacula. Very extensive contractile movements were very frequently observed in the pharynx, gullet and stomach. The arrangement of the muscles, by the action of which the polype protrudes and withdraws itself within the cell, appears, as far as I could trace them, similar to those in *Cellularia reptans* and *scruposa*. The greater number of specimens were provided with ovary-capsules, placed upon the thickened superior margin of the cells. In some specimens pro-

\* This is a mere conjecture thrown out for future investigation.

cured about the middle of October, these ovary-capsules were more or less filled with opake bodies (ovaries) of a slightly yellowish colour. Each of these bodies was composed of small nucleated cells inclosed in a membrane. The external surface of this membrane was in many of them provided with cilia in motion, causing some of them to perform a rapid rotatory motion within the ovary-capsules. These ova in the first stage of their growth adhere to the upper end of the lining membrane of the capsule. This lining membrane stretches across the aperture in the capsule, and also sends a reflection across the cell immediately below the ovum so as to inclose it in a kind of sac, leaving however, in the young ovum, a space between them. In the more advanced ova, this membranous partition was much thickened, especially at the central part, forming a considerable projection in the direction of the aperture in the capsule, and contained a number of nucleated cells. When the ovum enlarges so as to fill the interior of the capsule, it pushes this membranous partition before it. This membrane was observed in a few instances where the ova were fully formed to contract and relax at intervals, and in this way it may assist in the escape of the ovum. On detaching some of the ovary-capsules with the view of examining their contents under a high power, one of the ova was seen partially extruded from the aperture in the capsule. It was divided by a deep fissure into two unequal parts, the largest of which was nearly entirely outside the capsule (fig. 13 *a*). The extremity of the largest portion (fig. 13 *c*) was distinctly prolonged, more translucent than the rest of the ovum, and presented along its free edge a row of hairs resembling cilia, which, however, remained quite motionless, while along the whole of the rest of the external surface of both portions, except upon the edges of the fissure, cilia were in such vigorous action that it was impossible to distinguish them individually, and they produced the appearance of the rim of a wheel in very rapid rotation. After the lapse of an hour the fissure had extended through the whole body of the ovum, and the larger portion (fig. 13 *b*) being set free, swam about very actively in the water; but all this time the hairs attached to the prolonged anterior portion remained motionless. The smaller portion continued in the capsule, and performed very rapid rotatory movements. This was the only ovum I observed in the act of escaping from the interior of the capsule, but I had an opportunity of watching three other bodies exactly similar to the larger portion of the ovum already described, when examining other portions of the same polypidom. One of these had become fixed, by the hairs attached to the anterior extremity, to a minute portion of sea-weed, and the cilia were in active motion. When examined ten hours after,

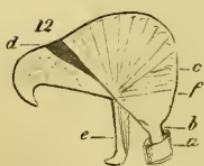
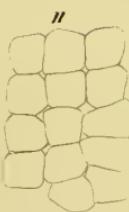
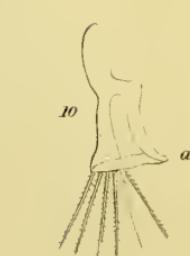
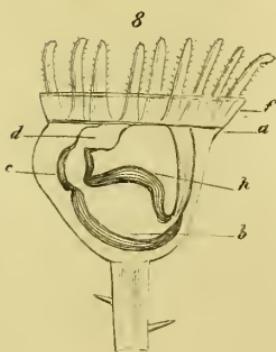
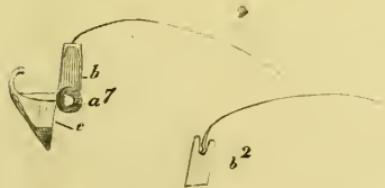
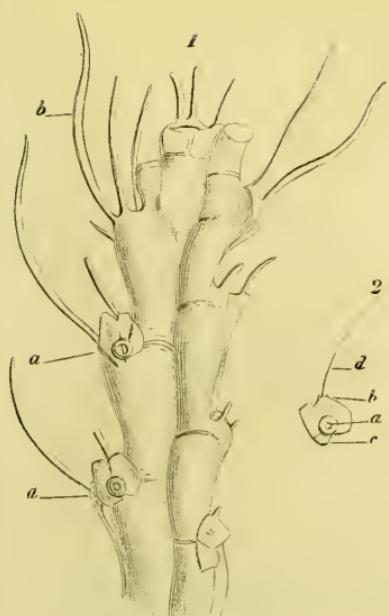
the cilia were acting very languidly. I saw another while swimming about become entangled by its cilia to the setæ projecting from the body of a small annelide. During the movements of the annelide, the hairs on the prolonged anterior extremity came in contact with some small fragments of sea-weed, and the annelide after some struggles detached itself from the ovum, which continued to adhere to the sea-weed. In all of these I never observed the least movement of the hairs attached to the anterior extremity. I was not able to ascertain that the smaller portion of the ovum left in the capsule underwent any change, as I presume it does, before it escaped from its interior. Several bodies, having one portion of their surface ragged and devoid of cilia, and in every other respect resembling the smaller portion of the ovum, and also other bodies exactly similar to the entire ovum, were observed swimming about; but as in all these cases the portions of the polypidom had been injured immediately before, and some of the ovary-capsules broken, it was presumed that these had been mechanically displaced from the capsules. The larger portions of the ova were, like the entire ova, composed of minute nucleated cells, and did not, as far as I could discover, possess any internal cavity.

In several specimens of *Cellularia reptans* and *C. scruposa*, and one specimen of *C. avicularis* procured at the same time, the ovary-capsules were filled with ova; in the two former of a deep orange colour, composed of nucleated cells, having the same number and arrangement of membranes and provided with cilia as in *Flustra avicularis*. Some of these ova were in rapid rotatory motion; others, as in *Flustra avicularis*, were motionless, though the cilia were acting, being kept quiescent by the more close apposition of the inclosing membrane. I did not succeed in observing the escape of any of these ova from their capsules. In many of the polype-cells of all of the above-mentioned polypes, dark red bodies composed of nucleated cells inclosed in a membrane were present. These nucleated cells are generally considerably larger than those entering into the formation of the ova in the ovary-capsules. The greater number of polype-cells contained one only of these bodies, and it was connected to the inner surface of the cell by a membrane having a number of detached nucleated cells of a light colour adhering to it. These bodies occupied different positions between the bottom and aperture of the cells, but in none were distinct ciliary motions observed. These bodies are also probably ova, and it is possible that more extended observations may enable us to detect cilia on their surface at a more advanced stage of development, though none in the present case were seen even on those lying at the aperture of the polype-cells. I have satisfied my-

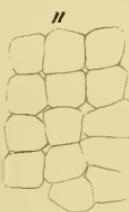
self that in the polypes mentioned above, the inner surfaces of the polype-cells, of the appendices of those processes described in the *Cellularia reptans* and *scruposa*, of the bird-head processes, of the spines, and of the canals running along the lateral surfaces of the polypidom in *Flustra avicularis*, are all lined by a fine membrane. This membrane in old specimens, and when the polypes are dead, often presents numerous and pretty large cells, generally of a pale colour, at other times having a slightly yellowish or brownish tinge, adhering to its free surfaces. In one specimen these cells had accumulated in such quantities within some of the spines in *Flustra avicularis*, as to produce considerable bulgings and excrescences. The growth and nutrition of the hard parts of the polypidom must be chiefly due to this membrane.

#### EXPLANATION OF PLATE XII.

- Fig. 1.* Magnified view of the posterior portion of the upper end of a branch of the polypidom in *Cellularia reptans*. It is slightly elevated on the left side, so that the polype-cells of that side are better seen than on the other.
- Fig. 2.* Three appendices to the cells in *Cellularia reptans*.
- Fig. 3.* Magnified view of four polype-cells of *Cellularia reptans* seen on the anterior surface.
- Fig. 4.* Magnified view of polype in *Cellularia reptans* when folded up in its cell.
- Fig. 5.* Magnified view of this polype when expanded.
- Fig. 6.* Magnified view of the anterior surface of the upper part of one of the branches of the polypidom in *Cellularia scruposa*. The polype-cells are in this drawing also more distinctly seen on one side than on the other.
- Fig. 7.* Magnified view of three appendices to the polype-cell in *Cellularia scruposa*; *b, b, bis*, views of the process bearing the hair-like prolongation in two different positions.
- Fig. 8.* Greatly magnified view of head and upper part of stalk in *Pedicellina echinata*.
- Fig. 9.* Greatly magnified view of the ciliated ova of *Pedicellina echinata*.
- Fig. 10.* Magnified view of polype in *Crisia chelata*.
- Fig. 11.* Magnified view of polype-cells in *Alcyonidium parasiticum*.
- Fig. 12.* Magnified view of bird-head process in *Flustra avicularis*.
- Fig. 13.* Magnified view of ova in *Flustra avicularis*.



10



12





RÉFLEXIONS ET OBSERVATIONS  
SUR LE TRAITEMENT  
DES  
RÉTRÉCISSEMENTS  
DE L'URÈTRE.

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PARIS - IMPRIMERIE DE BOURGOGNE ET MARTINET,  
rue Jacob, 30

(32)

RÉFLEXIONS ET OBSERVATIONS

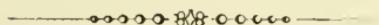
SUR LE TRAITEMENT

DES

RÉTRÉCISSEMENTS  
DE L'URÈTRE,

PAR

LE DOCTEUR J. BÉNIQUÉ.



PARIS.

LIBRAIRIE DE FORTIN, MASSON ET C<sup>ie</sup>,

1, PLACE DE L'ECOLE-DE-MÉDECINE;

Même maison, chez L. Michelsen, à Leipzig.

1845



## RÉFLEXIONS ET OBSERVATIONS

SUR

# LE TRAITEMENT DES RÉTRÉCISSEMENTS DE L'URÈTRE.



Préoccupés à juste titre des graves accidents qui sont souvent la conséquence des rétrécissements de l'urètre, les chirurgiens ont fait, pour éclairer l'histoire de cette affection, des recherches multipliées.

Loin d'avoir été stériles, ces travaux ont détruit beaucoup d'erreurs et enrichi la science de données importantes. Leur conclusion seule, c'est-à-dire la partie thérapeutique, me paraît laisser beaucoup à désirer.

Mon but n'est point ici de publier un traité dogmatique ; d'exposer la structure et la disposition anatomique de l'urètre, l'évolution des altérations et des phénomènes morbides qui peuvent affecter non seulement l'urètre, mais la vessie et les parties supérieures de l'appareil urinaire ; d'insister longue-

## 2 RÉFLEXIONS ET OBSERVATIONS SUR LE TRAITEMENT

ment sur les perturbations qu'apporte le rétrécissement dans la fonction dont le mécanisme est lésé et dans la santé générale. N'envisageant aujourd'hui que le côté essentiellement pratique de la question, j'essaierai de présenter les résultats auxquels j'ai été conduit par une expérience de dix années, convaincu qu'ils rendront beaucoup plus simple et plus facile la guérison, objet jusqu'ici de tant d'efforts.

Un grand nombre de méthodes ont été proposées pour guérir les rétrécissements de l'urètre ; mais le praticien peut-il sans quelque hésitation déterminer le choix qu'il doit faire entre elles ?

Lorsqu'il consulte les traités les plus récents de pathologie chirurgicale, y trouve-t-il des préceptes certains propres à le guider dans sa conduite auprès des malades ? Si, laissant de côté les traités généraux pour recourir aux monographies, il demande à chaque auteur ce qu'il faut penser de ces diverses méthodes, et surtout ce qu'il convient de faire dans un cas donné, chaque auteur lui répond par une théorie qui lui est propre ; chaque auteur a sur ce point sa thérapeutique : de là des opinions absolues, exclusives, plus ou moins ingénieuses sous le rapport scientifique, mais d'une médiocre utilité pour celui qui veut, avant tout, guérir son malade.

D'où vient cette incertitude ? De tous les systèmes qui ont été imaginés, aucun n'aurait-il paru assez avantageux pour mériter une préférence motivée ?

Il s'agit cependant d'une maladie très commune, parfois fort grave. Son origine, son développement,

les modifications variées qu'elle produit dans les tissus organiques, ne sont plus un mystère. Puis donc que des éléments du problème aucun ne fait défaut, les partisans de toutes les innovations que nous avons vues se succéder ne seraient-ils pas arrivés à des solutions contradictoires, parce qu'ils ont perdu de vue un principe qui devait les diriger dans leurs recherches ?

Les rétrécissements diffèrent beaucoup par leur forme et leur organisation anatomique, mais ils se ressemblent par leurs effets. Ils ont tous pour résultat commun de diminuer de plus en plus le diamètre de l'urètre ; de là deux ordres de symptômes.

Le premier comprend toutes les modifications de la miction jusqu'à la rétention complète de l'urine dans la vessie.

Le second embrasse tous les phénomènes morbides qui résultent de l'excrétion imparfaite du liquide urinaire. On sait, en effet, que la gêne apportée à l'accomplissement de cette fonction ne peut se prolonger sans léser les parois du conduit qui s'étend entre la vessie et l'obstacle. Car cette portion du canal est presque continuellement en contact avec une certaine quantité d'urine arrêtée derrière le rétrécissement ; ce liquide s'altère et irrite la membrane muqueuse, condamnée dès lors à un état d'inflammation chronique presque constant. De temps à autre, quelquefois sans cause apparente, le plus souvent par suite d'un excès de table ou de fatigue, l'inflammation passe à l'état aigu. Sous cette nouvelle

influence , la sécrétion purulente augmente au point de simuler un écoulement contagieux ; puis , au bout de quelques jours de repos et de régime , la douleur diminue ainsi que l'écoulement ; mais il est rare que ce dernier disparaisse complètement .

A ces désordres locaux il faut souvent ajouter une tristesse profonde et le dégoût de la vie , devenue insupportable par les souffrances et l'incapacité qu'ils entraînent avec eux .

Tels sont les principaux symptômes des rétrécissements de l'urètre . Comme leur développement est ordinairement lent et progressif , ils marchent presque à l'insu du malade , qui éloigne encore , soit par incurie , soit par appréhension d'un traitement qu'on lui a peint sous des couleurs très sombres , le moment où il consultera un médecin .

Ces effets divers sont la conséquence mécanique et physiologique de la disformité accidentelle de l'urètre ; et en rétablissant le diamètre naturel de ce conduit , on les fera nécessairement disparaître . Sur ce point tout le monde est d'accord ; mais pour arriver au but , les moyens varient , et , celui-ci une fois atteint , chacun se glorifie des succès obtenus en suivant son système , lui attribuant sur tous les autres une incontestable supériorité .

Mais en admettant que le résultat fut identique quelle que soit la méthode employée , ne faudrait-il pas encore se demander si toutes ces méthodes sont également bonnes ; si , par exemple , quelques unes d'entre elles n'exposent pas le malade à des accidents

plus ou moins graves ; si la guérison est également durable dans tous les cas ; enfin si les récidives ne présentent pas des caractères particuliers , selon qu'on a suivi tel ou tel traitement.

Fort compliquée en apparence , cette discussion se simplifie singulièrement , si l'on tient compte d'un principe très important : c'est que , de toutes les combinaisons thérapeutiques qui ont été proposées , aucune ne permet d'obtenir une guérison radicale , dans l'acception absolue de ce mot. Assurément , à la fin du traitement les symptômes de la maladie auront disparu ; mais en général cet état satisfaisant ne se maintiendra qu'à la condition que pendant un certain temps , et à des intervalles de plus en plus éloignés , on continuera à introduire dans l'urètre des instruments d'un volume convenable. Telle est du moins l'opinion unanime des praticiens les plus éclairés et les plus consciencieux.

En dehors de ce principe , toute appréciation thérapeutique est impossible. Je ne fais pas ici , remarquons-le bien , une réserve au profit de la méthode que j'emploie et dont je donnerai tout-à-l'heure la description ; car j'ai appris par expérience , et je le dirai plus tard , que si l'on peut espérer , pour les rétrécissements de l'urètre , des cures vraiment radicales , c'est elle qui offre le plus de chances de produire ce résultat.

Guérir un rétrécissement signifiera donc pour nous amener le malade à ce point que , délivré de tous les symptômes de la maladie , il puisse facilement s'in-

troduire une bougie de huit à neuf millimètres. De tous les traitements , celui-là sera le meilleur qui le plus sûrement et avec le moins de douleur opérera cette importante transformation.

Poser ainsi la question , c'est évidemment faire un grand pas vers la connaissance de la vérité. Dès lors le but étant bien défini , il devient facile pour chacun d'apprécier à leur juste valeur les divers moyens qui peuvent y conduire : aussi , dans cet opuscule , ne m'attacherai-je pas à critiquer les travaux de mes prédécesseurs. Je me bornerai à exposer le mode de traitement qui m'a été enseigné plus encore , je l'avoue , par l'expérience que par le raisonnement ; et je m'efforcerai de démontrer que s'il est très avantageux pour les malades , c'est parce qu'il satisfait avec une grande simplicité aux conditions réelles du problème.

La méthode que j'ai adoptée se rapproche beaucoup de celle qui est généralement connue sous le nom de dilatation. Appliquée à la guérison des rétrécissements , cette expression a successivement désigné des procédés fort différents. Le temps n'est pas loin de nous où Dupuytren , dans son service , consacrait aux individus affectés de cette maladie une salle particulière. Là , à mesure qu'ils entraient , on les soumettait en général à une loi commune. Aussitôt qu'une sonde avait pu pénétrer dans la vessie , elle y restait à demeure ; on ne la retirait que pour la remplacer par une plus grosse. On arrivait ainsi jusqu'à un diamètre de six à huit millimètres.

Enfin, pour rendre la guérison plus durable, on laissait la plus grosse sonde cinq à six jours dans l'urètre.

Pour ceux donc qui se reporteraient à cette époque, le traitement par la dilatation serait le séjour au lit, le repos absolu pendant un mois ou six semaines. A quoi il faut ajouter tous les accidents qui peuvent être la conséquence de cette pratique. Ils sont trop connus pour que j'en fasse le sujet d'une discussion approfondie; je me bornerai à consigner ici une observation qui ne me paraît pas entièrement dépourvue de vérité.

Les inconvénients qui résultent du séjour d'une sonde dans la vessie se produisent chez les divers individus en raison directe de la susceptibilité de leur système nerveux. Cette propriété vitale ne serait-elle pas jusqu'à un certain point sous la dépendance du genre de vie et des habitudes hygiéniques des malades? Toujours est-il qu'à plusieurs reprises ayant essayé d'appliquer en ville la dilatation permanente, j'en ai obtenu des résultats bien plus favorables que ceux que j'avais constatés dans les hôpitaux sur des hommes habitués en général aux plus rudes travaux. On conçoit aisément la répugnance que ce genre de traitement devait inspirer aux malades, et peut-être est-ce là l'origine de toutes les inventions par lesquelles on a essayé de le remplacer.

La pratique de la lithotritie a beaucoup contribué à améliorer l'emploi de la dilatation. On sait que pour le succès de cette opération il est nécessaire

de faire préalablement disparaître les strictures de l'urètre. Ce n'est pas que pour broyer la pierre on soit forcé d'employer des instruments volumineux ; je crois même avoir démontré que sous un très petit diamètre on peut leur donner une grande résistance, et s'assurer que celle-ci ne sera pas dépassée pendant l'opération ; mais il importe que les fragments et le détritus ne soient arrêtés dans leur sortie par aucun obstacle. Or, la sonde réagit d'une manière si fâcheuse sur la vessie déjà irritée par la présence du calcul, qu'on dut renoncer à la laisser à demeure dans l'urètre. On essaya donc la dilatation temporaire, en bornant à une heure ou deux le séjour de la bougie ; et comme les résultats de cette innovation furent satisfaisants, on l'étendit au traitement général des rétrécissements par la dilatation. Cette pratique est encore aujourd'hui la plus usitée. Quant à moi, après quelques essais de moyens mécaniques qui, dans leur application, ne justifièrent pas toutes mes espérances, je la suivis presque exclusivement jusqu'en 1839. Mais vers cette époque je fus conduit à modifier beaucoup mes idées ; et depuis près de six ans chaque jour m'a de plus en plus démontré l'utilité de la méthode que je vais exposer.

Je donnais des soins à un malade affecté d'un écoulement chronique peu abondant, mais fort ancien. Je soupçonnai l'existence d'une altération de l'urètre ; et en effet, une bougie de trois millimètres fut arrêtée à douze centimètres environ du méat par un obstacle d'une grande sensibilité. Puis une bougie de deux

millimètres l'ayant franchi, je me disposai à la laisser séjourner une heure; mais le malade se plaignit de ressentir une douleur très vive qui s'exaspérait au lieu de diminuer; et des spasmes nerveux étant venus justifier, du moins en partie, ces assertions, je me décidai à retirer l'instrument qui les provoquait.

Plusieurs fois je renouvelai les mêmes tentatives sans plus de succès. Je savais cependant que le seul moyen de faire disparaître l'écoulement était de dilater le rétrécissement; enfin, à la quatrième séance, voyant la douleur cesser aussitôt que je retirais la bougie, j'essayai d'en introduire une seconde qui était seulement plus grosse d'un quart de millimètre. Elle passa facilement. Le lendemain, nouvelle introduction de cette seconde bougie, qui fut suivie d'une troisième; et en continuant ainsi, c'est-à-dire sans laisser jamais les bougies plus d'une demi-minute dans l'urètre, j'arrivai sans le moindre accident, sans déterminer la plus légère irritation, à un diamètre de huit millimètres.

Ce fait me frappa beaucoup. A dater de ce moment je traitai ainsi presque tous les rétrécissements que je rencontrai, et aujourd'hui je crois pouvoir affirmer qu'aucune autre méthode n'offre autant d'avantages et aussi peu d'inconvénients.

Je n'ignore point toutes les objections que je dois soulever. On pensera d'abord que le passage d'une bougie à une autre plus grosse doit s'effectuer violemment; et pourtant personne plus que moi n'est

convaincu que , dans ces maladies , la violence est toujours nuisible , et retarde souvent la guérison au lieu de l'accélérer : aussi , avant de citer des faits à l'appui de mon opinion , j'ai hâte de dire comment je procède.

Pour ne point compliquer hors de propos cette discussion , je supposerai vaincues les premières difficultés du traitement. Étant donné , par exemple , un rétrécissement de trois millimètres , il s'agit de l'amener graduellement à une dilatation de huit à neuf millimètres.

Évidemment la bougie de trois millimètres ne pourra être remplacée par une autre plus grosse , qu'autant que leurs diamètres différeront seulement d'une très petite quantité. La division de dix millimètres en trente diamètres , d'après laquelle chaque bougie diffère de la suivante d'un tiers de millimètre , convenable seulement dans quelques cas faciles , rendrait souvent la méthode inapplicable. J'ai adopté comme suffisamment lente , le plus ordinairement , une progression par sixième de millimètre ; en sorte que l'intervalle de zéro à dix millimètres comprend soixante numéros également espacés.

Il faut d'abord soigneusement tenir compte du degré de facilité avec laquelle a été précédemment introduite la bougie de trois millimètres , qui correspondrait , dans la nouvelle division , au numéro dix-huit. Pour peu que cette opération ait été difficile en débutant par elle , on développerait de la douleur , de l'irritation , et tout progrès ultérieur serait interdit

pour le moment ; mais au contraire si l'on commence par faire pénétrer dans le rétrécissement trois ou quatre numéros inférieurs au numéro dix-huit , celui-ci passera facilement , et presque toujours il pourra être suivi du numéro dix-neuf. Loin d'être pour le malade une cause de douleur, l'introduction successive de bougies convenablement graduées rend infinitiment moins pénible le cathétérisme avec un instrument qui doit rencontrer quelque résistance. C'est un fait que la pratique seule peut rendre évident ; le démontrer plus amplement m'est complètement impossible.

Il n'est pas toujours nécessaire de suivre rigoureusement la progression par sixième de millimètre. Souvent on peut sans inconvenient franchir un ou deux numéros de la filière. L'habitude rend cette appréciation ordinairement facile ; mieux vaut toutefois excès de prudence qu'une trop grande précipitation.

Chez les malades qui n'ont encore subi aucun traitement, la dilatation s'opère en général avec une extrême simplicité ; et bien que je sache avec quelle excessive réserve il faut, en médecine, faire usage des termes absolus, je dois dire qu'ayant déjà soigné un grand nombre de sujets appartenant à cette catégorie, je n'ai pas rencontré une seule exception.

Ceux, au contraire , sur lesquels on a pratiqué diverses opérations, principalement si l'on a fait usage et abus de la cautérisation , se trouvent dans des conditions beaucoup moins favorables. Souvent

on est forcé de laisser les bougies plus ou moins longtemps dans l'urètre pour arriver sans violence à une dilatation de deux à trois millimètres. Parfois aussi on rencontre de temps à autre des points d'arrêt qui ne se laissent franchir qu'après plusieurs séances infructueuses. Il faut alors s'armer de patience, et multiplier la division des bougies.

La distinction que je viens d'établir entre les rétrécissements cautérisés antérieurement et ceux qui ne l'ont point été, cessera d'étonner, si l'on considère qu'après la cautérisation il existe nécessairement, au niveau du point rétréci, une couche plus ou moins épaisse de tissu inodulaire, et que ce tissu possède des propriétés toutes spéciales. Quel chirurgien n'a remarqué sa tendance constante à revenir sur lui-même ? Acquis depuis longtemps à la pratique, ce fait n'est-il pas mis à profit toutes les fois que l'on veut rétrécir les orifices de certains conduits fistuleux ? En vain cherche-t-on à étendre les brides, les cicatrices : elles se retractent lentement, mais avec une énergie proportionnée à l'épaisseur des parties qui ont été primitivement détruites ou enlevées.

Au contraire, lorsque les tissus ont conservé leur structure normale, ils ne peuvent, malgré leur élasticité, supporter une distension considérable sans éprouver une modification et perdre partiellement la propriété de réagir. Voyez les parois de l'abdomen après la grossesse ou la guérison d'une ascite, voyez la peau qui, pour recouvrir certaines tumeurs, s'est

agrandie à ce point qu'on est obligé d'en retrancher une partie.

Les conduits muqueux n'échappent point à cette règle. On sait quelles difficultés apporte à l'accouchement, chez les primipares, la résistance des parties externes de l'organe de la génération ; c'est même une des causes qui réclament fréquemment l'emploi du forceps. Au second accouchement les conditions sont différentes ; l'orifice vulvaire livre facilement passage à la tête du fœtus ; et cependant, remarquons-le bien, la distension antérieure qui a opéré ce changement n'a duré que quelques instants.

Je multiplierais inutilement les exemples à l'appui de cette proposition, à savoir : que les tissus sains, lorsqu'ils ont été soumis à une distension qui dépasse les limites de leur extensibilité naturelle, ne reviennent jamais complètement sur eux-mêmes, tandis que le tissu inodulaire conserve constamment sa propriété rétractile.

Si nous appliquons aux rétrécissements de l'urètre ces principes généraux, deux objections se présentent :

1<sup>o</sup> La cautérisation ne produit pas une escarre et du tissu de cicatrice.

2<sup>o</sup> Les tissus qui forment le rétrécissement ne sont plus à l'état normal.

Remarquons d'abord que le caustique, en contact avec les parties vivantes, obéit tout simplement aux lois de l'affinité chimique. Lorsqu'on le voit produire

une escarre toutes les fois qu'il est appliqué sur une plaie , sur un ulcère , sur une membrane muqueuse, n'est-on pas en droit de conclure qu'il agit de même à la surface de l'urètre ?

La seconde objection n'est certainement pas dépourvue de fondement , et c'est précisément pour cela que la dilatation ne donne pas toujours une guérison absolue , c'est-à-dire à l'abri de toute récidive ; mais ces récidives seront d'autant plus à craindre que le tissu de l'urètre aura été plus altéré. Tous nos soins doivent donc tendre à faire perdre le moins possible à ce tissu ses propriétés normales. En nous écartant de cette règle , nous devons nécessairement nous attendre à ce que , si la maladie se reproduit, comme c'est l'ordinaire , elle se représentera à nous caractérisée par des complications qui rendront le second traitement beaucoup plus pénible que le premier.

Depuis que j'ai mis ces idées en pratique , j'ai eu fréquemment à me louer de l'emploi des bougies métalliques , principalement quand je rencontrais des cas très difficiles. Mais alors , fidèle à mon principe d'éviter par-dessus tout la violence, je faisais souvent usage d'instruments dont les diamètres différaient à peine d'un douzième de millimètre. Je n'essaierai point de déterminer *à priori* dans quelles occasions conviennent les bougies élastiques ou métalliques ; mon seul guide à ce sujet, c'est la sensibilité particulière des malades. Les premières développent-elles pendant leur introduction une douleur insolite, je me servirai des secondes ; et réciproquement. Une

seule tentative n'est pas toujours suffisante pour juger lesquels sont les moins douloureux , des instruments flexibles ou rigides. Approprier la courbure de ceux-ci à chaque individu, voilà le point essentiel. Si cette indication a été négligée , on provoquera de la douleur, qu'avec un peu plus de soin on eût souvent évitée.

Remarquons encore à ce propos combien il est avantageux d'introduire à chaque séance plusieurs bougies graduées d'une manière presque insensible. C'est surtout lorsque l'on se sert des instruments rigides que la perfection du manuel opératoire devient absolument nécessaire. Assurément le chirurgien , depuis qu'il a commencé le traitement, a dû observer très exactement la conformation particulière du malade, le nombre , la nature et la position des divers obstacles ; mais comment ne pas reconnaître que trois ou quatre opérations faciles lui permettant d'étudier, pour ainsi dire , de nouveau ces détails importants , de modifier encore , s'il y a lieu, la forme des bougies , de déterminer avec un soin minutieux la direction qu'il doit leur donner, le placeront dans des conditions singulièrement favorables au moment où il va peut-être rencontrer quelques difficultés ? Le succès du cathétérisme ne dépend-il pas presque toujours de l'observation des nuances les plus minimes ?

Quelques exemples m'aideront à préciser les points qui , dans cette exposition , auraient pu paraître trop peu explicites.

## PREMIÈRE OBSERVATION.

Rétrécissement fort ancien : guérison en six semaines au moyen des bougies graduées. Une bougie conservée accidentellement dans l'urètre pendant une heure et demie, occasionne une légère inflammation.

M. N —, habitant les colonies françaises, vint à Paris vers les premiers jours de juin 1844 pour s'y faire traiter d'une affection des voies urinaires qui depuis plusieurs années le tourmentait cruellement.

Ce malade, âgé de soixante-deux ans, était doué d'une bonne constitution et d'un tempérament bilieux et sanguin. Il avait eu plusieurs blennorrhagies qui guérirent dans l'espace de deux à trois mois. En 1850, il contracta un nouvel écoulement ; mais celui-ci résista à tous les moyens employés pour le combattre. L'année suivante, M. N — s'aperçut qu'il urinait moins librement, et cette difficulté s'accrut progressivement, à ce point qu'en 1852 les besoins d'uriner se manifestaient presque tous les quarts d'heure ; ils n'étaient qu'incomplètement satisfaits, et nombre de fois chaque nuit ils interrompaient le sommeil.

Cependant l'écoulement n'avait point cessé : si parfois il diminuait, peu de temps après il reparaissait avec plus d'abondance que jamais.

Ces alternatives faisaient le désespoir du malade. Comme toutes ses souffrances dataient de la dernière blennorrhagie, il ne doutait pas qu'elles ne fussent entretenues par la même cause. A peine se laissait-il aller à l'espoir qu'elle allait disparaître, qu'aussitôt ses illusions s'évanouissaient, et il retombait dans le déconseil.

C'est ainsi qu'il vécut jusqu'en 1841. Je ne dirai point toutes les médications auxquelles il eut recours pendant ce laps de temps : remèdes internes, injections de toute espèce, et même avec du sublimé à haute dose, etc.

Suffisamment éclairé sur la nature de la maladie, j'introduisis dans l'urètre une bougie cylindrique très souple, de trois millimètres de diamètre. Elle s'arrêta à onze centimètres et demi et provoqua un peu de douleur. Une bougie de deux millimètres rencontra la même résistance, et ne doutant plus de l'extrême étroitesse de l'obstacle, ce que du reste les symptômes fonctionnels rendaient plus que probable, je laissai reposer le malade.

Le lendemain, j'essayai le cathétérisme avec des bougies souples et déliées, en changeant plusieurs fois, au lieu d'insister, quand elles ne paraissaient pas devoir pénétrer.

Après cinq minutes de tentatives infructueuses, je m'arrêtai : il sortit une gouttelette de sang; le malade avait très peu souffert. Je passe rapidement sur les détails qui ne se rattachent pas directement à la thèse que je soutiens. Je me bornerai donc à dire qu'à la quatrième séance seulement, après des essais variés, multipliés, mais dans lesquels je n'employai jamais la violence, je franchis l'obstacle avec une bougie dont le diamètre était d'environ un millimètre et quart. Elle était un peu serrée par le rétrécissement; néanmoins pour la retirer je n'eus besoin que d'une faible traction.

Le jour suivant, le malade pensait reconnaître dans son état une légère amélioration. Après quelque hésitation, j'introduisis dans la vessie la même bougie que la veille; puis la retirant aussitôt, je la remplaçai par une seconde. Celle-ci, il est vrai, n'était plus grosse que d'un sixième de millimètre. A la troisième séance de dilatation, les deux

premières bougies furent suivies d'une troisième; puis je laissai de côté la première, la regardant comme inutile.

Pendant huit jours, je continuai ainsi, supprimant la bongie inférieure à mesure que j'étais parvenu à en introduire une plus grosse, et n'ayant besoin, pour obtenir ce dernier résultat, que de deux introductions préparatoires. Je ne me servais plus que d'instruments cylindriques.

L'écoulement avait quelque peu diminué; l'émission de l'urine se faisait facilement et à des intervalles assez éloignés; les nuits étaient bonnes, et M. N., qui dans l'origine n'avait accepté qu'avec restriction les espérances que je lui donnais, commençait à ne plus douter du succès, lorsqu'un matin, au moment où je venais de faire pénétrer très facilement dans la vessie la dernière bougie de la séance (trois millimètres et demi environ), on introduisit dans l'appartement où nous étions une personne qui venait rendre visite au malade. Celui-ci ferma sa robe de chambre et je le quittai.

Le lendemain je compris au premier coup d'œil qu'un changement notable était survenu dans son état. Il devança mes questions en me disant qu'il n'allait pas bien. La veille, il avait éprouvé de fréquentes envies d'uriner; elles s'étaient prolongées pendant la nuit; il avait dormi d'un sommeil agité et s'était réveillé cinq ou six fois. J'avais annoncé que, les premières difficultés vaincues, tout irait de mieux en mieux, et les faits venaient donner à mes paroles un démenti formel.

A quelle cause devais-je attribuer cette brusque modification dans la marche du traitement? M. N. était naturellement très sobre; il avait souffert si longtemps et si cruellement, il désirait si vivement guérir qu'il se faisait

un plaisir de rendre plus sévère le régime que je lui avais prescrit : aussi quand je lui demandai s'il avait fait quelque excès de table, de marche, etc., je m'attendais à une réponse négative ; elle fut telle. Me rappelant alors la visite qui nous avait surpris la veille au matin, j'appris qu'elle s'était prolongée pendant une heure et demie, et M. N. avait conservé sa bougie, attendant pour la retirer le moment où il serait seul.

Notre traitement fut suspendu pendant deux jours, au bout desquels les symptômes inflammatoires avaient complètement disparu ; mais je n'hésitai point à reconnaître l'origine de leur développement dans le hasard qui avait fait garder une heure et demie la dernière bougie, bien que pendant ce temps elle n'eût provoqué aucune douleur.

Quelle que fût au reste la probabilité de mon opinion, la suite lui donna une sorte de confirmation. A dater de ce moment, je repris le traitement et le continuai sans le moindre accident, gagnant régulièrement tous les jours quelque peu en diamètre, un sixième de millimètre environ. C'est ainsi que j'arrivai, sans avoir provoqué ni douleur ni écoulement de sang, à une dilatation de huit millimètres et demi : le méat ne permettait pas l'introduction d'instruments plus volumineux. Je regardai la dilatation comme suffisante, d'autant plus que la bougie pénétrait dans la vessie sans rencontrer le moindre arrêt.

Sept semaines après notre première entrevue, je pris donc congé du malade. Il urinait parfaitement bien ; il n'avait plus d'écoulement ; il ne ressentait plus la moindre douleur. Je lui recommandai expressément d'introduire une bougie tous les mois ou tous les deux mois, et en lui donnant ces conseils j'avais la certitude qu'ils seraient ponctuellement suivis. De temps à autre je

reçois de ses nouvelles ; elles sont toujours très satisfaisantes.

L'extrême simplicité de cette cure, sa marche un peu lente, mais toujours progressive, la faculté laissée au malade pendant toute sa durée de ne rien changer à son genre de vie habituel, pourraient faire croire que j'ai choisi à dessein un cas extrêmement facile. Telle est cependant, à très peu d'exceptions près, l'histoire de tous les malades que j'ai soignés avant qu'ils eussent subi d'autres traitements.

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## DEUXIEME OBSERVATION.

Incontinence d'urine causée par un rétrécissement. Guérison en six semaines. Point de récidive au bout de quatre ans, quoique le malade n'ait rien fait pour prévenir le retour de sa maladie.

En 1840, M. B. vint me consulter pour une maladie fort douloureuse, disait-il, mais peut-être encore plus pénible par l'état de malpropreté auquel il était condamné. Il était affecté d'incontinence d'urine. Il éprouvait des besoins d'uriner très fréquents ; l'urine sortait alors difficilement et en petite quantité ; puis après cette miction incomplète, soit le jour, soit pendant la nuit, elle s'écoulait spontanément.

D'une constitution robuste, M. B. menait une vie très active autant par goût que par sollicitude pour les intérêts importants qui lui étaient confiés. Il était âgé de trente-neuf ans.

L'hypothèse d'une paralysie partielle du système nerveux était si peu fondée, que, sans m'y arrêter, je demandai quelques détails sur les symptômes morbides qui avaient dû antérieurement affecter l'appareil urinaire.

Un premier écoulement, contracté à une époque très éloignée, avait été soigné avec peu de méthode; il avait duré près de huit mois. Après sa guérison, M. B. avait joui pendant un an ou deux d'une parfaite santé; survinrent alors, de temps à autre, des écoulements que le malade désignait sous le nom d'échauffements et qu'il contractait avec une extrême facilité. Vers cette époque, l'émission de l'urine se fit plus lentement; bientôt ce liquide ne sortit plus que par un jet délié, parfois interrompu, et avec une faible impulsion qui devenait presque nulle avant que le besoin d'uriner fût complètement satisfait. M. B. éprouvait fréquemment des douleurs dans la région du périnée, une sensation de pesanteur vers l'hypogastre. Il supporta cet état avec beaucoup de patience, craignant surtout qu'un traitement ne l'empêchât de vaquer à ses affaires. Enfin depuis six mois environ l'incontinence d'urine s'était déclarée.

Tous ces accidents étaient évidemment causés par un rétrécissement de l'urètre. La difficulté progressivement croissante opposée par lui à la sortie de l'urine avait amené l'accumulation et le séjour forcé de ce liquide dans la vessie, et les fibres musculaires qui enveloppent le col de cette cavité, soumises à une distension presque continue, avaient perdu en partie leur contractilité. L'urine sortait alors, selon l'expression que l'usage a consacrée, par regorgement.

Je constatai, avec une bougie cylindrique de quatre millimètres, un rétrécissement situé à douze centimètres

du méat. Dans la première séance, après quelques tâtonnements plus longs que douloureux, je parvins à introduire dans la vessie une bougie d'un millimètre et quart légèrement effilée à son extrémité; je la retirai aussitôt.

Le lendemain, nouvelle introduction de la même bougie, qui fut suivie d'une seconde.

A la douzième séance, nous avions fait un très notable progrès : une bougie cylindrique de quatre millimètres franchissait l'obstacle sans difficulté. Une grande amélioration était survenue dans l'état du malade. Non seulement il n'avait plus d'incontinences d'urine, mais ses besoins d'uriner étaient peu fréquents, et il pouvait les satisfaire librement et complètement.

A partir de cinq millimètres j'adoptai les bougies métalliques, au grand contentement du malade qui trouvait leur introduction moins douloureuse, et c'est avec elles que, sans le moindre accident, et assurément après avoir causé bien peu de douleur, je terminai le traitement, en six semaines après l'avoir commencé.

Non seulement M. B. ne fut nullement empêché de se livrer à ses affaires, mais, par suite de ses occupations, il y eut souvent plusieurs jours d'intervalle entre nos séances. Ici, comme en bien d'autres circonstances analogues, je ne remarquai nullement que cette suspension fût nuisible et qu'elle nous fit perdre du terrain.

M. B. avait vu disparaître successivement tous les symptômes de sa maladie ; il s'introduisait facilement une bougie voluminouse. Je l'engageai à répéter de temps à autre cette opération, et je lui fis comprendre que cette précaution seule pouvait le préserver d'une rechute. Il me promit bien qu'il s'acquitterait de ce soin. Trois mois après je le rencontraï. Il était fort satisfait de son état,

mais il avait négligé l'introduction des bougies. Craignant alors que de nouvelles recommandations n'eussent pas plus d'efficacité que les premières, je le pria de m'avertir dès qu'il éprouverait les plus légers symptômes de son ancienne maladie.

Un an environ après notre traitement, il me dit qu'il urinait toujours très bien, mais qu'il croyait ressentir un peu de pesanteur dans la région hypogastrique. Cependant j'introduisis sans la moindre difficulté une bougie métallique de neuf millimètres et je me bornai à donner quelques conseils hygiéniques.

Depuis lors M. B. n'a cessé de jouir de la plus parfaite santé. Ainsi donc, atteint d'un rétrécissement fort ancien et très étroit, il a négligé impunément pendant quatre années toutes les précautions qui pouvaient assurer sa guérison, et elle ne s'est point encore démentie.

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### TROISIÈME OBSERVATION.

Gonflement très douloureux de l'épididyme entretenu pendant cinq ans par un rétrécissement. Celui-ci est traité par les bougies graduées ; les douleurs de l'épididyme disparaissent avant la fin du traitement ; la tumeur diminue progressivement.

Les rétrécissements de l'urètre sont quelquefois accompagnés de diverses complications qui rendent plus précieux encore l'avantage d'arriver à une dilatation suffisante, sans développer des inflammations dont les conséquences pourraient être fort graves. A ce propos je citerai l'observation suivante.

Au mois d'octobre 1841, je fus appelé en consultation, avec un des médecins les plus éclairés de Paris, auprès de M. J. Agé de vingt-neuf ans, d'une constitution délicate, affaibli par une affection chronique des voies digestives, ce malade était en outre cruellement tourmenté par une tumeur située vers l'extrémité supérieure du testicule droit. Depuis cinq ans et demi, l'épididyme de ce côté était le siège d'un gonflement considérable; à chaque instant des douleurs très vives s'y faisaient sentir; elles s'exaspéraient à la moindre fatigue, et M. J. en était réduit à n'oser faire à pied le plus court trajet.

Pour remédier à cet état fâcheux, M. J. avait consulté plusieurs chirurgiens qui conseillèrent les antiphlogistiques, les applications résolutives, le repos au lit pendant plusieurs mois; mais ces moyens n'amènèrent aucune amélioration.

Voici quelle était l'origine de cette maladie. En 1855, M. J. contracta une blennorhagie. Elle fut très douloureuse et sa durée se prolongea beaucoup. L'année suivante, il s'aperçut qu'il urinait difficilement. Un médecin, consulté par lui, introduisit dans l'urètre une sonde d'argent et rencontra, environ à onze centimètres du méat, un rétrécissement qu'il ne put franchir. Cette exploration fut douloureuse; il survint un gonflement considérable du testicule droit.

Deux mois après, j'eus occasion de voir M. J. Il souffrait toujours beaucoup du testicule; il me raconta ce qui lui était arrivé. Je l'engageai à suivre un traitement pour le rétrécissement de l'urètre aussitôt que les symptômes inflammatoires auraient disparu. Mais, vivement impressionné par une première opération douloureuse, il mani-

festa pour toute espèce de cathétérisme l'aversion la plus absolue.

Cinq années s'écoulèrent sans apporter un grand changement dans son état. L'urine sortait lentement, par un jet souvent interrompu, néanmoins sans de trop grandes difficultés. Un régime d'une excessive sévérité, et dont M. J. ne se départit pas un seul instant, l'avait probablement préservé des accidents de rétention d'urine. L'épididyme avait conservé le volume d'une grosse noix ; des douleurs aiguës s'y faisaient toujours sentir. Entravant M. J. jusque dans les moindres détails de la vie, cette dernière affection devenait à ses yeux bien autrement importante que le rétrécissement de l'urètre, et c'était d'elle surtout qu'il nous demandait de le délivrer.

A quelle cause devions-nous attribuer l'acuité et la persistance des douleurs dans une tumeur aussi ancienne ? La propagation d'une inflammation de l'urètre vers l'épididyme est excessivement fréquente. Développée dans le premier, une irritation, quelle qu'en soit la cause, envalait très souvent le second. M'appuyant sur ce principe, je me demandai si, chez M. J., l'état de l'urètre ne formait pas le principal obstacle à la guérison de l'orchite. L'existence d'un rétrécissement, son influence ordinaire sur la partie postérieure du conduit, rendaient à mes yeux cette hypothèse probable. Elle était encore justifiée par l'inutilité des traitements antérieurs, dirigés cependant par des chirurgiens fort habiles. En suivant la même route, je ne devais certainement pas obtenir plus de succès. Je proposai donc de commencer par faire disparaître le rétrécissement.

Au premier abord, cette opinion était difficilement acceptable ; mes propres arguments se tournaient contre

moi, et si, dans le traitement ordinaire de cette maladie, on voit souvent survenir l'inflammation du testicule, à plus forte raison, chez M. J., devait-on redouter un pareil accident. Cependant, convaincu par l'expérience de l'innocuité de la méthode que je suis, j'insistai, et je parvins à faire partager la confiance que j'avais moi-même dans les moyens dont je conseillais l'emploi.

Une bougie à pointe mousse n'ayant pu, quoique très étroite, pénétrer dans le rétrécissement, fut laissée en contact avec lui, et après deux séances de trois ou quatre heures elle finit par le franchir. On épargna ainsi au malade la douleur qu'auraient peut-être provoquée des tâtonnements multipliés pour arriver plus rapidement au même résultat. Cette pratique fort sage diffère entièrement du séjour des bougies prolongé dans un but de dilatation. Le corps étranger se trouve en contact avec une membrane muquineuse ordinairement saine dans le premier cas, toujours malade dans le second. Libre dans l'un, il est serré dans l'autre avec plus ou moins d'énergie.

Au reste, dès que le passage fut libre, je commençai la dilatation, et les bougies furent toujours aussitôt retirées qu'introduites. Souvent trois ou quatre bougies se succédèrent, jamais plus. Redoutant par-dessus tout l'influence de ce traitement sur l'affection de l'épididyme, j'interrogeais sans cesse le malade; je l'engageais à s'étudier lui-même, et je lui avais fait comprendre combien un accident de ce genre serait aggravé s'il pouvait en quelque sorte marcher à notre insu. J'appris, à ma grande surprise, que pendant cinq ans M. J. n'avait jamais moins souffert du testicule que depuis le commencement de notre traitement. Je continuai donc avec moins

d'appréhension. Mais, sans tenir compte de l'extrême facilité avec laquelle marchait la dilatation, je m'imposai dans ce cas particulier la règle de parcourir successivement tous les degrés de la filière. Chaque séance fut marquée par un léger progrès, et nous arrivâmes à un diamètre de neuf millimètres avec une régularité qui, quelques années plus tôt, m'aurait beaucoup surpris.

Ce traitement fut terminé à l'aide des bongies métalliques, et voici pour quel motif. La dilatation du rétrécissement n'était point douloureuse : cependant, à partir de sept millimètres, je remarquai que les bongies élastiques occasionnaient par leur frottement sur la membrane muqueuse, principalement dans la partie antérieure de l'urètre, une sensation désagréable. J'essayai les bongies métalliques, et le malade trouva ce changement tellement avantageux qu'il me pria instamment de renoncer pour toujours aux premières.

Désormais l'état de M. J. n'a cessé d'être on ne peut plus satisfaisant. La tumeur de l'épididyme a considérablement diminué de volume, et elle ne le fait nullement souffrir, bien qu'il ait renoncé au régime plus que sévère suivi pendant cinq ans avec tant de persévérance et si peu de succès.

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#### QUATRIÈME OBSERVATION.

Rétention d'urine causée par un rétrécissement. Traitement par les bongies graduées. Après sa guérison, le malade néglige pendant plus de quatre ans l'introduction des bongies sans être atteint de récidive.

Appelé en 1859 auprès de M. V., je le trouvai dans un

état d'exaltation difficile à décrire. Depuis trente-six heures il n'avait pas uriné, et il éprouvait des douleurs tellement intolérables qu'au moment où j'arrivai, décidé à ne pas les endurer plus longtemps, il écrivait ses dernières volontés. Je m'efforçai de le calmer en lui promettant une prompte délivrance.

Après bien des tâtonnements, après avoir essayé divers instruments, au bout de vingt à vingt-cinq minutes je fis pénétrer dans la vessie une bougie légèrement effilée et qui n'avait guère qu'un millimètre de diamètre. Je la retirai presque aussitôt, mais très lentement. Elle fut suivie d'un jet d'urine très délié, qui cependant permit à la vessie de se débarrasser en grande partie du liquide qui la distendait.

M. V. put alors répondre aux questions que je lui adressai au sujet de sa maladie. En 1817, il contracta une première blennorrhagie très douloureuse, très intense et qui se prolongea pendant près d'un an. D'autres écoulements survinrent à diverses époques, mais le jet de l'urine ne fut sensiblement modifié qu'en 1827. Il diminua progressivement jusqu'en 1857, et alors M. V. éprouva de telles souffrances qu'il fut forcé de consulter un médecin. Celui-ci essaya vainement d'introduire une sonde métallique dans la vessie ; la douleur fut très vive et le sang sortit en grande abondance.

Naturellement peu soigneux de sa santé, le malade se déconseigna promptement en voyant l'insuccès de cette première tentative. Mais, quoique la sonde n'eût pu franchir l'obstacle, il urinait avec plus de facilité ; il attribuait ce résultat à l'abondante hémorrhagie provoquée par le cathétérisme. Malheureusement cette amélioration ne fut que passagère ; bientôt il vit reparaître dans toute leur

intensité les symptômes de sa maladie, et, soit négligence, soit appréhension d'un traitement douloureux, il attendit pour m'appeler auprès de lui qu'il y fût constraint par l'impossibilité absolue d'uriner.

Avant de songer à traiter le rétrécissement, il fallait prévenir le retour des accidents. Introduire dans la vessie une sonde d'un diamètre suffisant pour assurer l'écoulement de l'urine, c'eût été une opération difficile et d'autant plus pénible pour le malade qu'il sortait à peine d'une crise longue et douloureuse. Je me bornai pour le moment à replacer la petite bougie dans l'urètre. Elle était assez fortement serrée par le rétrécissement. Lorsque le besoin d'uriner se fit sentir, M. V. la retira de deux ou trois centimètres; puis il la repoussa ainsi que je lui avais recommandé.

Je le vis le soir sur les dix heures. Le pouls était calme; point de soif; sentiment général de lassitude, mais la douleur avait presque entièrement disparu. Peu de temps après il s'endormit.

Le lendemain je le trouvai dans un état très satisfaisant. Pendant le sommeil la bougie était sortie de l'urètre, mais l'urine s'écoulait, quoique bien lentement, avec assez de facilité. Dans la crainte de causer de l'irritation, je m'abstins de toute tentative de cathétérisme.

Le jour suivant, je commençai à attaquer la cause de la maladie par une dilatation progressive du rétrécissement. J'usai de beaucoup de modération, surtout dans les premières séances. Les progrès, lents d'abord, devinrent bientôt plus rapides, et six semaines après l'accident pour lequel j'avais été appelé, M. V. était parfaitement en mesure de s'introduire lui-même une bougie de neuf millimètres. J'ajouterai qu'au bout de huit jours envi-

ron, il vit disparaître le suintement purulent de l'urètre qui avait été constant pendant un grand nombre d'années.

Pendant tout le cours de ce traitement, à l'exception des premiers jours, M. V. put librement vaquer à ses affaires. Je lui avais seulement recommandé de manger peu, de s'abstenir de vin pur et d'éviter de marcher de manière à se fatiguer.

M. V. appréciait vivement le bien-être dont il jouissait; il savait qu'à sa négligence surtout il fallait attribuer les cruelles souffrances dont le souvenir était encore bien récent. Pour assurer sa guérison, il devait simplement passer de loin en loin une bougie dans l'urètre, et je ne doutais pas qu'instruit à ses dépens, il ne s'acquittât ponctuellement de cette petite opération, désormais très facile et exempte de douleur. Il n'en fit rien; je le rencontre très souvent et toujours insouciant de l'avenir.

Chez les malades appartenant à la classe aisée, cette disposition d'esprit est heureusement peu commune. Déjà près de cinq années se sont écoulées sans que M. V. soit atteint d'une récidive dont je l'ai maintes fois menacé et que je persiste à regarder comme très probable.

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## CINQUIÈME OBSERVATION.

Écoulement chronique entretenu pendant deux ans par un rétrécissement de l'urètre. Dilatation du rétrécissement, puis guérison de l'écoulement par les injections de ratanhia pratiquées immédiatement après l'introduction d'une bougie.

Un des symptômes les plus saillants, les plus caractéristiques des affections de l'appareil urinaire, c'est la tristesse, le découragement, le dégoût de la vie, qu'elles inspirent aux malades.

Loin d'être en rapport direct avec la gravité des altérations organiques, cet effet moral atteint souvent un développement considérable par la seule persistance d'une maladie légère, mais qui pendant longtemps résiste à une foule de remèdes. Les exemples de ce genre se rencontrent chaque jour dans la pratique : je ne bornerai à en citer deux ou trois pris au hasard.

M. D., d'un tempérament lymphatique, avait toujours joui d'une très bonne santé. A l'âge de vingt-trois ans, il contracta une blennorrhagie qui fut traitée méthodiquement (application de sanguines, bains, diète, boissons délayantes, puis bols de cubèbe et de copahu). Au bout de deux mois et demi, l'écoulement disparut complètement.

Trois ans plus tard, nouvelle blennorrhagie pour laquelle on suivit le même traitement sans le moindre succès. Les symptômes inflammatoires céderent assez promptement, mais l'écoulement persista. Pour le combattre, M. D. employa pendant deux ans, et avec une rare persévérance, une foule de moyens dont l'énumé-

ration seule serait fort longue. Toutes les formules d'injections furent successivement essayées à diverses doses. L'écoulement s'arrêtait quelquefois, mais il reparaissait presque aussitôt.

L'esprit sans cesse préoccupé de sa maladie, M. D. était tombé dans l'état moral le plus déplorable. Il était arrivé à se persuader que l'affection de l'urètre réagissait sur le cerveau ; ses travaux étaient suspendus ; il se regardait comme incapable de toute étude, à cause de l'impossibilité de fixer son attention.

J'écoutai patiemment le long récit de la maladie, de ses symptômes et des divers essais de traitement. J'émis l'opinion qu'un rétrécissement de l'urètre pouvait bien être le seul obstacle à la guérison ; et en effet, une bougie cylindrique de quatre millimètres rencontra, à dix centimètres du méat, un point d'arrêt qu'elle ne put franchir qu'avec un certain frottement. J'annonçai à M. D. que dans un mois, selon toute apparence, il serait débarrassé de son affection ; et j'étais autorisé à lui parler ainsi, par le nombre de cas semblables que j'avais précédemment observés. Mais son imagination était tellement frappée, que cette assertion lui parut téméraire ; et croyant devoir m'éclairer davantage, il m'énuméra de nouveau tout ce qu'il avait souffert et tout ce qu'il avait tenté en vain.

Le rétrécissement offrit très peu de résistance ; il céda facilement, et, au bout de trois semaines, M. D. s'introduisait sans aucune douleur une bougie de neuf millimètres.

Jusque là l'écoulement n'avait point été modifié. N'étant plus entretenu par une cause pour ainsi dire matérielle, il aurait probablement disparu de lui-même au bout d'un certain temps. Pour accélérer ce résultat, je

recommandai à M. D. d'introduire matin et soir la bongie, et d'injecter dans l'urètre, au moment où il la retirait, une solution d'extrait de ratanhia. Huit jours après, l'écoulement avait entièrement cessé.

Les personnes qui ont été le plus vivement impressionnées par la durée d'une maladie s'abandonnent souvent avec la même exagération à un autre ordre d'idées lorsque la guérison leur paraît probable. M. D., très content du changement opéré dans sa position, était tourmenté du désir de s'assurer si sa guérison était durable et définitive. Dans ce but, malgré mes conseils, il se livra à divers excès dont la conséquence fut une urétrite aiguë et un écoulement abondant. Ce léger accident lui donna une leçon utile. Nous employâmes quelques jours à combattre l'inflammation, puis, à l'aide des bougies et des injections de ratanhia, nous nous rendîmes facilement maîtres de l'écoulement, qui, cette fois, disparut sans retour.

Continuées d'abord tous les deux ou trois jours, les injections furent bientôt supprimées ; les bougies ne furent introduites qu'à des intervalles de plus en plus éloignés, et désormais M. D. fut délivré de tous les tourments qui l'avaient si longtemps affligé.

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## SIXIÈME OBSERVATION

Écoulement chronique sans rétrécissement appréciable. Après avoir pendant deux ans résisté à une foule de remèdes, il disparaît sous l'influence des injections de ratanhia combinées avec l'introduction des bougies.

M. P., âgé de vingt-neuf ans, réunissait au plus haut

degré tous les éléments qui peuvent caractériser une excellente constitution. De vingt à vingt-cinq ans, il contracta plusieurs blennorrhagies dont il se guérit assez facilement, sans cependant renoncer jamais à une vie de plaisirs, de veilles et de fatigues. A vingt-six ans, nouvelle blennorrhagie. Mais, cette fois, les moyens précédemment employés échouèrent complètement. M. P. prit du cubèbe et du copahu à des doses énormes, et qui, chez tout autre individu, auraient probablement altéré les fonctions digestives; il fit longtemps des injections avec des astringents végétaux, des sels d'argent, de plomb, de zinc. Il conserva toujours un suintement purulent très peu abondant, mais constant. Au bout d'un an, il remarqua un nouveau symptôme morbide. Les érections, quoique fréquentes, devinrent moins rigides. Dans le coït, l'éjaculation arrivait immédiatement, et la sensation était presque nulle.

Très affligé de cet état, auquel il pensait sans cesse, M. P. tomba dans une tristesse profonde. Il changea de genre de vie, et chercha à se créer des goûts et des habitudes d'un autre âge. Généralement il était taxé d'hypochondrie.

Cependant la santé générale n'avait point souffert; les excès de table ne modifiaient presque point l'affection de l'urètre, et lorsque, après avoir parcouru l'Italie pendant six mois, M. P. vint me consulter, je fus frappé d'un singulier contraste entre cette constitution brune, robuste, vigoureuse, et l'expression de chagrin profondément empreinte sur le facies.

Je commençai par examiner l'urètre. J'espérais rencontrer, comme je l'ai vu si souvent, un rétrécissement qui, sans produire encore de trouble notable dans la

miction, aurait entretenu, dans la partie supérieure de l'urètre, une inflammation chronique. Cependant une bougie cylindrique de quatre millimètres pénétra dans la vessie sans rencontrer aucun arrêt, sans provoquer la moindre douleur. Une seconde bougie de huit millimètres fut introduite avec presque autant de facilité. Je fus très contrarié de ce résultat; car, en général, j'ai remarqué que les écoulements chroniques très anciens, mais peu abondants, guérissent avec d'autant plus de rapidité qu'ils sont entretenus par un rétrécissement plus appréciable.

Convincu cependant qu'il s'agissait ici d'une inflammation chronique de la partie supérieure de l'urètre, je crus devoir employer le moyen qui m'a toujours réussi en pareil cas.

J'annonçai à M. P. que le traitement serait probablement long; que je ne pouvais en fixer le terme, même approximativement, et je l'engageai à faire matin et soir, avec de l'extrait de ratanhia, des injections qui seraient précédées de l'introduction d'une bougie de neuf millimètres.

Pendant la première quinzaine, il ne survint aucune amélioration; souvent même l'écoulement paraissait plutôt augmenté que diminué. Bientôt il se réduisit à l'état où il était avant le traitement.

Un peu plus tard, M. P. alla passer quelques jours à la campagne; il marcha beaucoup, ne fit point d'injection, et à son retour à Paris il m'annonça avec le plus grand découragement qu'il lui était survenu un écoulement très abondant, tel qu'il ne l'avait pas vu depuis deux ans. Je persévérai cependant dans la marche que j'avais adoptée, et j'eus tout lieu de m'en applaudir. L'écoulement diminua rapidement, et après être resté

à peu près stationnaire pendant trois semaines , il disparaît complètement , environ deux mois après le commencement de notre traitement.

Singulièrement satisfait, M. P. reprit avec entraînement la vie de plaisirs à laquelle il avait renoncé bien à regret ; il chercha même à se dédommager du temps perdu, et ne se fit faute d'aucun excès. Non seulement, par suite de ce brusque changement dans ses habitudes , il ne fut pas atteint d'écoulement , mais il avait recouvré complètement l'exercice des fonctions génitales, dont l'affaiblissement l'avait jeté pendant deux ans dans le désespoir le plus profond.

Dans un écrit consacré aux rétrécissements de l'urètre, cette observation pourrait paraître déplacée. Mais le suintement purulent accompagne presque constamment les rétrécissements de l'urètre , dont il est même , ainsi que nous l'avons dit, un des symptômes. Si, dans la méthode que je suis , il disparaît le plus souvent de lui-même , il montre parfois une rare ténacité , surtout quand on a rendu l'inflammation des tissus plus profonde en laissant séjourner des sondes dans l'urètre. Alors sa persistance désole le malade , en même temps qu'elle devient pour le médecin une difficulté pratique.

Telle est probablement la raison pour laquelle beaucoup de chirurgiens, tout en reconnaissant que la cautérisation est un traitement fort imparfait des rétrécissements, emploient cependant cette médication dans l'espoir de tarir la source des écoulements opiniâtres.

Malheureusement il est presque impossible de déterminer avec assez de précision le point qui fournit la suppuration , pour limiter convenablement l'action du

nitrate d'argent. Plusieurs chirurgiens se décidèrent à l'introduire dans l'urètre en quelque sorte au hasard. Un de mes amis et confrères me disait même avoir obtenu de très bons effets de cette méthode, qu'il appelait cautérisation intercurrente. Il conduisait le porte-caustique dans la région la plus profonde de l'urètre ; il mettait le nitrate d'argent à découvert ; puis il retirait lentement l'instrument en lui imprimant quelques légers mouvements de rotation.

Si je ne m'étais interdit ce genre d'argumentation qui consiste à énumérer les accidents causés par les méthodes que je condamne, les exemples se présenteraient en grand nombre pour démontrer les inconvénients de cette pratique. Je citerais tels individus dont la santé, jusque là robuste et vigoureuse, fut pour bien des années détruite par des cautérisations que motivaient seulement des maladies légères. J'ai préféré me borner à exposer, peut-être un peu longuement, un moyen fort simple, nullement dangereux, et qui m'a toujours réussi ; car je chercherais vainement dans mes souvenirs un seul cas où, après la guérison des rétrécissements, c'est-à-dire lorsqu'ils ne faisaient plus saillie dans l'urètre, les injections combinées avec l'introduction des bougies n'aient pas obtenu un succès complet.

J'ai rencontré seulement trois malades chez lesquels un écoulement purulent et chronique, bien qu'il fût entretenu par un rétrécissement, était cependant accompagné de certaines complications qui m'empêchèrent de les guérir. Les deux premiers présentaient à un assez haut degré la difformité connue sous le nom d'hypospadias. En outre la paroi inférieure de l'urètre s'aminçait extrêmement en arrivant à l'ouverture antérieure, qui elle-

même était fort étroite. Je reculai, peut-être à tort, devant la pensée d'agrandir cette dernière et d'accroître ainsi la difformité naturelle.

Le troisième, d'un esprit assez inquiet, vint me consulter dernièrement pour un écoulement chronique fort rebelle. Deux fois il avait été récemment cautérisé sans succès. Je constatai un rétrécissement de trois millimètres, dans lequel j'introduisis une bougie cylindrique d'une extrême souplesse. Deux jours après, je fis suivre la même bougie d'une seconde, qui ne rencontra, quoique également très souple, aucune résistance. A peine un léger frottement m'indiqua-t-il le moment où elle franchissait l'obstacle.

Le lendemain, j'appris avec surprise que le malade ne pouvait plus uriner. Depuis la cautérisation, il avait bien éprouvé quelques accidents de ce genre ; mais nous dûmes attribuer celui-ci à l'introduction de la bougie. Il fit devant moi des efforts inutiles ; la verge se gonflait ; la vessie était pleine de liquide, mais il n'en sortit pas une goutte. Cependant la bougie pénétra dans la vessie aussi facilement que la veille ; je la retirai, et le malade évacua près d'un litre d'urine.

Il ne s'attendait pas, me dit-il, à ce que l'introduction des bougies produirait de tels effets. Je lui répondis très naïvement que je l'ignorais moi-même ; que cela m'étonnait, surtout en pensant combien cette opération avait été facile ; j'ajoutai que, selon toute probabilité, il serait promptement à l'abri de ces inconvénients..., mais depuis lors je ne l'ai plus revu.

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## SEPTIÈME OBSERVATION.

Inflammation chronique de la partie la plus reculée de l'urètre. Même traitement que dans la précédente observation.

M. D., d'une constitution assez robuste, mais épuisée par des excès, ressentait habituellement dans la vessie des douleurs vagues qui se propageaient dans l'urètre et correspondaient au périnée. Les érections étaient fréquentes et incomplètes; elles rendaient toujours les souffrances plus vives. Il s'écoulait habituellement de l'urètre un liquide dont l'abondance variait beaucoup, mais qui laissait constamment sur le linge des taches d'un gris plus ou moins foncé. Depuis sa maladie, qui datait de deux ans et demi, M. D. avait cru remarquer un affaiblissement général du système nerveux. Il éprouvait de fréquents maux de tête. Il attribuait tous les accidents aux érections, et pour les éviter il s'imposait toute espèce de privation.

Une bougie de six millimètres, après avoir facilement traversé l'urètre, dont la sensibilité me parut très vive, s'arrêta brusquement au niveau de la prostate. J'essayai d'introduire une seconde bougie également flexible et cylindrique, mais dont j'avais rendu la conrbnre très prononcée. Elle rencontra le même arrêt que la première, puis, après une légère hésitation, elle pénétra dans la vessie. Je la retirai aussitôt.

Trois semaines furent consacrées à amener, par des introductions successives, le passage facile d'une bougie de huit millimètres et demi. Dans les derniers temps, nous

avions adopté les bougies métalliques. Je m'étais attaché avec grand soin à leur donner une courbure convenable, et le malade en trouvait l'usage si peu douloureux, qu'il se les introduisait lui-même, de préférence aux instruments flexibles.

Jusque là, l'état de M. D. n'avait subi pour ainsi dire aucune modification. Il était toujours tourmenté par l'écoulement et les érections douloureuses. Je prescrivis matin et soir une injection avec l'extrait de ratania, l'une d'elles, au moins, devant être chaque jour précédée de l'introduction d'une bongie volumineuse.

Après dix jours de ce traitement, le malade se reposa pendant trois jours. Il éprouvait, me dit-il, une notable amélioration. Il reprit l'usage du même moyen, et je l'engageai à persévérer pendant un temps qui probablement serait assez long, mais en lui faisant comprendre qu'il n'était nullement nécessaire d'agir sans interruption.

Au bout de trois mois, nous obtîmes un succès pour ainsi dire complet. L'écoulement avait disparu, et les érections avaient repris leur état normal.

J'engageai M. D. à ne pas renoncer trop brusquement aux soins qui avaient rétabli sa santé, et à introduire de temps à autre la bongie, réservant les injections pour le cas où quelque rechute les rendrait de nouveau nécessaires.

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## HUITIÈME OBSERVATION.

Obstacle à l'introduction des bougies, ayant son siège au col de la vessie ; écoulements purulents très fréquents. Même traitement que dans la précédente observation.

M. N., âgé de trente-cinq ans, d'une bonne constitution, n'avait jamais eu d'autre maladie qu'une affection de l'appareil urinaire déjà fort ancienne, et pour laquelle il venait me demander des conseils.

Depuis neuf à dix ans il éprouvait derrière le pubis une douleur, non pas très aiguë, mais presque constante.

Après avoir été à la selle, souvent même après l'émission de l'urine, il rendait fréquemment par l'urètre un liquide visqueux, transparent. Sous l'influence des causes les plus légères survenaient de temps à autre des écoulements purulents qui ne guérissaient que difficilement, en sorte que leur durée excédait souvent l'intervalle qui les séparait.

Pour modifier cet état fâcheux, consécutif à plusieurs blennorrhâgies, divers moyens avaient été employés sans succès, et comme je m'apprêtais à explorer l'urètre, M. N. me dit que chez lui on ne pouvait pas pénétrer dans la vessie.

En effet, une bougie de six millimètres arriva jusqu'au col sans la moindre difficulté; là, elle fut brusquement arrêtée par un obstacle qui paraissait infranchissable, mais peu sensible; car, en exerçant sur ce point une légère pression, on ne provoquait aucune douleur.

J'essayai vainement d'introduire dans la vessie d'autres bougies; droites ou courbes, cylindriques ou coniques,

elles rencontraient toujours la même résistance et ne paraissaient point, quel que fût leur diamètre, s'engager dans un rétrécissement.

Cependant le malade urinait librement; les dernières gouttes de l'urine s'écoulaient à la vérité lentement, avec peu d'impulsion; mais évidemment la modification de la miction n'était nullement en rapport avec la difficulté que présentait l'introduction des instruments d'un petit diamètre. Je fis de nouveaux essais avec une bougie de trois millimètres dans laquelle j'avais introduit un mandrin uniquement pour lui donner des courbures variées.

Enfin, à la quatrième séance, je pénétrai dans la vessie. Cette fois, le mandrin s'arrêtait à deux centimètres de l'extrémité de la bougie; quant à sa forme, je la représenterai assez exactement en disant qu'il offrait deux courbures dirigées en sens inverse et dont l'antérieure se rapprochait beaucoup du quart d'un cercle de quatre centimètres de diamètre.

Les recherches multipliées auxquelles j'avais dû me livrer avaient été fort peu pénibles pour le malade, qui ressentit au contraire une douleur assez vive au moment où l'obstacle fut dépassé. Presque aussitôt je retirai la bougie. Sans être positivement serrée dans le col de la vessie, elle donnait la sensation d'un frottement réel.

Dès lors, j'employai à peu près le même moyen pour l'introduction de bougies progressivement plus volumineuses. La dilatation s'opéra lentement, mais sans difficulté. A partir de quatre millimètres et demi, je pus supprimer le mandrin. J'avais soin seulement que les bougies fussent très recourbées à leur extrémité, ce qui ne les empêchait pas d'hésiter un peu avant d'entrer dans la vessie.

Au bout d'un mois, M. N. s'introduisait facilement des

bougies de neuf millimètres ; je l'engageai à combiner avec cette petite opération les injections de ratanhia. Il les pratiqua pendant six semaines, non pas d'une manière continue, mais avec quelques alternatives de repos. Voulant alors apprécier avec plus d'exactitude l'état de sa maladie, je lui fis suspendre pendant huit jours l'introduction des bougies et les injections. Quand je le revis, il m'annonça tout d'abord combien il était satisfait de sa santé. Il n'éprouvait plus la moindre sensation douloureuse. L'émission de l'urine était franche et régulière. L'écoulement purulent avait depuis longtemps cessé. Quant au suintement visqueux, considérablement réduit, il apparaissait uniquement lorsque M. N. faisait de grands efforts pour aller à la selle. Enfin je m'assurai, en pratiquant le cathétérisme, qu'une bougie cylindrique de neuf millimètres pénétrait facilement dans la vessie sans presque hésiter en franchissant le col de cet organe.

Je m'efforçai de démontrer au malade que sa guérison n'était ni complète ni définitive; que de lui seul il dépendait désormais de la consolider en ne renonçant pas brusquement à l'usage des bongies et des injections de ratanhia.

En lui donnant ces conseils, j'insistai d'autant plus que son père avait été atteint d'une affection fort grave de l'appareil urinaire, sur laquelle j'eus des renseignements incomplets, mais qui devait évidemment avoir son siège au col de la vessie.

Or, plusieurs faits constatés dans les mêmes familles me portent à croire que l'hérédité exerce quelque influence sur ces maladies, ou, en d'autres termes, que

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certains individus apportent en naissant une prédisposition aux maladies du col de la vessie.

Ainsi, par exemple, je connais trois frères dont l'aîné, après deux blennorrhagies, fut atteint d'un rétrécissement situé à sept centimètres du méat et d'un écoulement abondant de fluide prostatique accompagné d'un gonflement si considérable du col de la vessie, qu'on ne put jamais faire pénétrer les bougies dans l'intérieur de cette cavité.

A vingt et un ans, le second frère avait déjà un rétrécissement à sept centimètres et une disposition particulière du col de la vessie qui présentait un obstacle presque insurmontable à l'introduction des bougies.

Enfin le troisième contracta, à dix-huit ans, un premier écoulement contagieux, qui fut traité avec d'autant plus de soin qu'on avait attribué à la négligence des deux autres malades les accidents qui les avaient frappés. Les bains, les cataplasmes, les boissons délayantes furent employés avec persévérance. A deux reprises on appliqua trente saignées. Les préparations de cebèbe et de copalhu ne furent administrées qu'à faibles doses, et seulement après la disparition presque complète des symptômes inflammatoires. Malgré toutes ces précautions il survint, environ dix mois après la blennorrhagie, de la difficulté à uriner, par suite de laquelle on constata, comme chez les deux autres malades, un rétrécissement à sept centimètres du méat et une tuméfaction du col de la vessie.

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## NEUVIÈME OBSERVATION.

Rétrécissement datant de près de vingt ans. Récidives opiniâtres après plusieurs traitements par la cautérisation. On essaie la dilatation en laissant les bougies une heure ou deux dans l'urètre ; elles causent une irritation qui pendant plusieurs mois rend tout progrès impossible. Le rétrécissement est dilaté au moyen des bougies graduées.  
Insucès de la scarification.

M. N., donné d'un force musculaire vraiment athlétique et d'un système nerveux très développé, était âgé de quarante ans lorsqu'il vint me consulter.

L'origine de la maladie remontait à plus de vingt ans. Entré fort jeune au service militaire, il contracta plusieurs bleuorrhagies qui guérirent lentement. Dix-huit mois plus tard, il commença à éprouver de la difficulté à uriner. Il s'adressa à Antoine Dubois, qui constata un rétrécissement de l'urètre, et le traita par les sondes à demeure, dont le diamètre fut progressivement porté à neuf millimètres. Au bout d'un an, et probablement par suite de la négligence du malade, le rétrécissement se reforma. A cette époque la cautérisation de l'urètre était prônée par ses adeptes avec un enthousiasme qu'explique en partie un perfectionnement réel apporté dans la pratique de cette opération. Malgré les conseils d'Antoine Dubois, qui avait conçu pour lui une véritable amitié, M. N. recourut à cette méthode. Il fut cautérisé nombre de fois et avec une grande énergie. La guérison ne fut également que temporaire.

Un second traitement semblable ayant été suivi d'une nouvelle rechute, M. N., fort découragé, préoccupé des

soins qu'il était incessamment obligé de prendre de sa santé, toujours menacé d'une rétention d'urine complète, donna sa démission du service militaire, dont il se jugeait impropre à supporter les fatigues.

Il vécut ainsi quelques années, portant toujours sur lui une bougie sans le secours de laquelle il se serait trouvé souvent dans l'impossibilité absolue d'uriner. Puis la révolution de 1830 arriva, et en même temps se présenta pour M. N. une occasion de rentrer dans la carrière qu'il avait quittée, et pour laquelle il conservait toujours un amour très ardent; mais, convaincu que son affection était incurable, il refusa des offres très avantageuses.

En 1852, M. N. subit un nouveau traitement par la cautérisation. Il éprouva encore un soulagement momentané, puis le rétrécissement se reforma plus intense que jamais. Plusieurs années se passèrent, pendant lesquelles il n'urinait qu'avec le secours des bongies.

Enfin, trouvant sa position intolérable, bien que fort désabusé sur la valeur d'une méthode qui avait été employée sur lui avec tant de persévérance, M. N. voulut encore y recourir, espérant au moins obtenir un adoucissement passager à ses maux. Mais une nouvelle difficulté se présenta. Il fallait d'abord amener le rétrécissement à ce point que l'introduction du porte-caustique fût possible. Adoptant la marche qui avait été précédemment suivie, le chirurgien laissa une heure et demie dans l'urètre une bougie conique d'environ deux millimètres de diamètre. Puis il essaya de lui en substituer une autre un peu plus grosse. Vains efforts : la résistance était insurmontable. Pendant plusieurs mois, on continua les mêmes tentatives sans le moindre succès. L'emploi des sondes à demeure fut proposé au malade ; mais il s'y re-

fusa, redoutant l'excitation nervouse qui chez lui se développait avec une grande rapidité.

D'après ce simple exposé, que j'ai rapporté aussi succinctement que possible, on comprendra quelles difficultés devait présenter ici la dilatation; et s'il avait pu me rester quelques doutes à ce sujet, ils eussent bientôt disparu. Voulant, en effet, me faire apprécier exactement son état, M. N. me montra une bougie d'un millimètre et demi qu'il portait toujours sur lui, et qu'il introduisait assez facilement. Puis, parmi celles que je lui présentai, il en choisit une autre conique, de deux millimètres et quart, et qu'en raison de son peu de flexibilité je regardais comme défectueuse. Il l'engagea dans l'obstacle, et il la poussa avec une violence que je n'aurais certes pas osé employer moi-même.

Je vis alors qu'elle était serrée dans le rétrécissement avec une énergie vraiment extraordinaire. Je craindrais d'évaluer approximativement en poids la force que je dus employer pour la retirer; car, tout en restant au-dessous de la vérité, je serais probablement taxé d'exagération. Jamais je n'avais rencontré un rétrécissement d'une aussi grande dureté, et quand je me décidai à traiter ce malade selon ma méthode ordinaire, il n'était assurément permis de douter du succès.

Je commençai par choisir une série de bongies graduées d'une manière presque insensible, d'une médiocre flexibilité, et surtout cylindriques; car les instruments coniques, employés comme corps dilatants, m'ont toujours paru occasionner plus de douleur et d'irritation que les autres. Puis, à chaque séance, j'introduisis trois ou quatre bongies, en commençant toujours par celles qui devaient passer très facilement; je les retirais immédiatement. Au bout de dix jours, nous avions fait un très

notable progrès et dépassé le diamètre de la bougie conique qui, la première fois, n'avait pu franchir l'obstacle, malgré l'extrême violence avec laquelle M. N. l'avait poussée. Cependant notre marche n'avait point été régulière, et plusieurs fois nous dûmes renoncer à dépasser le point atteint dans la précédente séance.

A partir de cinq millimètres, je commençai à me servir des bougies métalliques. Elles me permirent d'examiner avec plus de soin l'état de l'urètre. Le principal obstacle était à onze centimètres et demi ; un peu plus loin je rencontrai bien encore un frottement anormal, mais je constatai avec plaisir que le col de la vessie ne paraissait pas avoir participé à la maladie.

Arrivés à six millimètres, nous fûmes arrêtés pendant quatre séances sans pouvoir faire le plus léger progrès ; puis enfin, nous dépassâmes ce point d'arrêt, et la dilatation devint plus facile. J'évitais avec grand soin toute cause d'irritation, je ne procédais que par des nuances insensibles, et je laissais souvent reposer le malade pendant sept à huit jours.

Nous avions obtenu une dilatation de sept millimètres ; le rétrécissement offrait de moins en moins de résistance ; déjà, à plusieurs reprises, M. N. m'avait exprimé combien il regrettait de n'avoir pas connu dix ans plus tôt les avantages de cette méthode ; enfin, tout annonçait une terminaison prochaine et satisfaisante de notre traitement, lorsqu'il fut brusquement interrompu.

M. N. avait contracté en 1856 une maladie syphilitique. Méconnue d'abord, cette affection amena le développement, dans la région inguinale, d'un abcès du plus fâcheux caractère, compliqué de trajets fistuleux très profonds. Presque toute l'épaisseur des parois abdominales

était depuis longtemps envahie, lorsque, heureusement, M. N. appela à son secours un chirurgien fort habile qui arrêta les progrès du mal. Sous l'influence d'une médication rationnelle et énergique, la plaie finit enfin par se cicatriser, mais avec une perte de substance considérable.

Or, dans l'intervalle de nos séances de cathétérisme, probablement par suite d'une marche trop prolongée, exercice dans lequel M. N. était infatigable, il arriva que la cicatrice se déchira. Bientôt l'ouverture, qui n'était tapissée que d'une très faible conche de tissu cellulaire, s'agrandit d'une manière menaçante, et j'engageai M. N. à consulter le chirurgien dont il avait reçu précédemment des soins si éclairés.

Deux mois de repos au lit n'avaient pas encore permis d'obtenir une complète cicatrisation. Pendant ce temps le rétrécissement avait été tout-à-fait négligé. M. N. remarqua qu'il urinait déjà moins bien; on lui proposa la scarification, qui fut acceptée immédiatement.

Malgré l'extrême habileté du chirurgien qui la pratiqua, cette opération ne réussit point. Il s'écoula beaucoup de sang; il survint presque aussitôt un gonflement de la verge tellement considérable, qu'il ne se dissipa complètement qu'au bout de trois semaines. Vers cette époque, M. N. partit pour la campagne, afin d'y consolider sa convalescence.

A son retour à Paris, après six mois d'interruption, nous reprîmes notre traitement. Pendant son séjour à la campagne, le malade avait de temps à autre introduit dans le rétrécissement quelques bougies, mais il avait à peine pu atteindre quatre millimètres; il regrettait fort de

n'avoir pas en à sa disposition des instruments métalliques, dont il trouvait l'usage beaucoup moins douloureux.

En suivant la même marche que par le passé, nous arrivâmes à des résultats semblables, c'est-à-dire à faire pénétrer facilement dans la vessie des bougies de sept millimètres. Malheureusement, des affaires importantes forcèrent M. N. à s'absenter. Depuis deux ans, je n'ai pas eu occasion de l'examiner, mais je doute beaucoup que le bien-être dont il jouissait se soit maintenu; car la dilatation n'avait pas été portée assez loin pour combattre avec efficacité le retour d'une maladie aussi opiniâtre.

En rapportant cette observation, que j'ai abrégée autant que possible, j'ai voulu seulement montrer par un exemple frappant les conséquences fâchées attachées à la pratique de la cautérisation de l'urètre, et les ressources que peut offrir la simple introduction des bougies. Dans ce cas, assurément fort difficile, l'extrême division des instruments successivement employés me permit seule d'arriver à un résultat que des circonstances fortuites, et surtout la négligence du malade, ont probablement rendu incomplet jusqu'à ce jour.

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#### DIXIÈME OBSERVATION.

Rétention d'urine complète causée par un rétrécissement qui se reproduisait constamment après de nombreuses cautérisations. Guérison en trois mois, d'abord par les sondes à demeuré, puis par les bougies graduées.

M. C., d'un tempérament bilieux, était arrivé à cinquante et un ans, ayant toujours mené une vie très active,

dont une grande partie avait été consacrée à de longs voyages.

Dès l'âge de vingt-cinq ans, il commença à éprouver de la difficulté à uriner. Depuis cette époque, il consulta plusieurs chirurgiens ; ils constatèrent un rétrécissement sur lequel, à diverses reprises, tant à Paris qu'en Angleterre, furent pratiquées de nombreuses cautérisations. Mais n'ayant obtenu de ces divers traitements que des améliorations passagères, M. C. s'était résigné à supporter patiemment sa maladie.

Maintes fois déjà il avait été atteint de rétentions d'urine complètes qui, fort heureusement, n'avaient jamais duré plus de cinq à six heures, lorsque, en 1842, le même accident survint. N'urinant que goutte à goutte, M. C. essaya inutilement d'introduire dans la vessie une petite bougie qui ordinairement franchissait l'obstacle assez facilement ; il prit un bain, et à dater de ce moment l'écoulement de l'urine fut entièrement supprimé.

Je trouvai ce malade dans une grande agitation. La rétention durait depuis vingt-quatre heures. Pouls fréquent ; sueurs visqueuses ; la région hypogastrique rendait un son mat dans une assez grande étendue. Après bien des tentatives infructueuses, après avoir employé successivement plusieurs bougies, toujours d'un petit diamètre, mais de forme et de résistance variées, je parvins à en faire pénétrer une beaucoup plus profondément que toutes les autres précédemment essayées. Je sentais que, si elle eût eu un peu plus de roideur, elle aurait probablement dépassé le rétrécissement, et, avant de me décider à la retirer, je conduisis dans l'urètre, jusqu'au niveau de l'obstacle, une sonde d'argent de trois millimètres de diamètre, longue de seize centimètres et ou-

verte à ses deux extrémités. Dans l'antérieure, j'avais préalablement engagé la partie externe de la bougie, qui se trouvait désormais renfermée dans un conduit rigide, de nature à empêcher qu'elle ne se repliât en deçà du rétrécissement; puis, tenant d'une main la sonde invariablement fixée, je donnai à la bougie une nouvelle impulsion: elle pénétra dans la vessie, et je retirai la sonde d'argent, désormais inutile.

Cette bougie (du diamètre d'un millimètre trois quarts) était très fortement serrée dans le rétrécissement. Je la retirai au bout de quelques instants, espérant que l'urine s'écoulerait: il ne sortit pas une goutte de ce liquide.

Cependant le malade souffrait beaucoup, non pas des longues tentatives de cathétérisme, mais de la distension considérable de la vessie et de l'insuccès de nos efforts pour le soulager. Je pris alors une sonde flexible du plus petit diamètre; je l'introduisis dans l'urètre. Elle s'engagea dans le rétrécissement, sans pouvoir le franchir. J'augmentai sa roideur en faisant glisser dans son intérieur, jusqu'à deux centimètres environ de son extrémité, un petit mandrin filiforme légèrement recourbé, en sorte qu'il représentait un arc appartenant à une circonférence de vingt-deux centimètres de diamètre, et ce moyen me permit enfin de pénétrer dans la vessie. Je retirai le petit mandrin; l'urine s'écoula, d'abord par une succession de gouttelettes, puis par un jet extrêmement délié, et qui ne s'arrêta qu'an bout de vingt minutes.

Bien avant ce temps, le malade avait éprouvé un soulagement considérable. En pareil cas, j'ai presque toujours remarqué que la douleur cessait pour ainsi dire aussitôt que sortaient les premières gouttes d'urine. Peut-être faut-il attribuer ce fait à ce que la certitude

qu'éprouvent les malades d'être promptement débarrassés de leurs souffrances , met fin à leurs angoisses et à leur anxiété.

Le lendemain , je trouvai M. C. dans un très bon état L'urine s'était facilement écoulée par la petite sonde que j'avais laissée à demeure. Celle-ci n'était plus serrée dans le rétrécissement. Je la retirai , et je fis pénétrer successivement à sa place plusieurs bougies graduées , dont la dernière avait environ deux millimètres de diamètre. Mais , à mesure qu'elles franchissaient l'obstacle , j'éprouvais une sensation particulière qui me confirmait de plus en plus dans la pensée que le rétrécissement opposerait à la dilatation une grande résistance. Le malade avait très peu souffert. Je le quittai avec l'espoir qu'en raison du léger élargissement obtenu dans cette séance , il urinerait naturellement jusqu'au lendemain.

Trois heures après , il me fit appeler. Depuis mon départ , il n'avait pas uriné , et il avait inutilement essayé d'introduire la petite sonde. Je la fis cependant pénétrer dans la vessie sans difficulté ; mais cette fois , tenant compte de l'ancienneté de la maladie , des nombreuses cantérisations qui avaient été pratiquées , et de la dureté que j'avais cru reconnaître dans le rétrécissement , je me décidai à laisser les sondes à demeure jusqu'à ce qu'elles eussent atteint un diamètre de trois millimètres. Nous y arrivâmes au bout de trois jours. Le malade urinait assez librement ; je le laissai reposer , et le cinquième jour après l'accident pour lequel j'avais été appelé , nous commençâmes la dilatation au moyen des bougies graduées.

La résistance de l'obstacle , la susceptibilité nerveuse du malade , me forcèrent , dans le principe , à n'avancer

que très lentement. A la cinquième séance , nous n'avions encore obtenu que trois millimètres et demi. Je fis usage, à partir de quatre millimètres , des bougies métalliques : elles paraissaient moins douloureuses, et causer moins d'irritation que les autres. Je termiuerai rapidement l'histoire de ce traitement , qui dès lors ne présenta rien de remarquable. Je retrouve seulement dans mes notes qu'à cinq millimètres et à sept millimètres et demi , je rencontrais une résistance si grande , que pendant trois ou quatre séances je ne pus faire aucun progrès.

Vers la fin du troisième mois , M. C. ne ressentait plus aucune espèce de douleur. Il s'introduisait facilement une bougie métallique de neuf millimètres de diamètre. Je l'engageai à répéter de temps à autre cette petite opération ; et il est probable qu'il aura suivi mes conseils , car, depuis lors , deux ans se sont écoulés , et je ne l'ai point revu.

Ce traitement paraîtra peut-être très long. Je me bornerai à remarquer que les difficultés avaient été fort augmentées par l'ancienneté de la maladie et par l'emploi réitéré du nitrate d'argent.

Quant au malade , non seulement il n'éprouva aucun accident , mais , à l'exception des premiers jours , consacrés à le préserver d'une rétention d'urine complète , il vit sa santé s'améliorer graduellement , sans qu'il eût à prendre d'autre soin que de se rencontrer quelques minutes avec moi tous les jours ou tous les deux jours.

## ONZIEME OBSERVATION.

Rétrécissement fort ancien traité plusieurs fois par la cautérisation. Guérison obtenue en deux mois et demi par les bougies graduées.

M. B., d'une haute stature, d'un tempérament sanguin, avait toujours joui d'une santé générale très satisfaisante : cependant il était tombé dans la plus profonde tristesse par suite d'une affection de l'urètre, dont désormais il désespérait de guérir.

A partir de sa première jeunesse, il avait contracté tant de blennorrhagies, qu'il en ignorait presque le nombre. A trente ans il commença à éprouver de la difficulté à uriner. Il consulta un chirurgien, qui, après avoir constaté plusieurs rétrécissements, les traita par des cautérisations profondes et répétées. Deux ans après, la maladie était plus intense que jamais. Nouveau traitement dont les effets ne furent pas plus durables.

M. B. était fort découragé. Non seulement il n'urinait qu'avec une peine extrême, mais depuis qu'il avait été cautérisé il ressentait dans la région du périnée une douleur assez vive et presque constante. On lui proposa alors la scarification. La première incision fut très douloureuse ; pendant huit jours il ne put supporter l'introduction de la plus faible bougie, et il renonça à ce moyen comme à la cautérisation.

Quand M. B. vint me consulter, il était âgé de quarante-deux ans. Malgré toutes les apparences d'une constitution très robuste, il ne vivait que de privations et se croyait condamné à souffrir toujours.

Je m'efforçai de chasser de son esprit ces tristes préoccupations en lui promettant une guérison peut-être fort lente, mais qui me paraissait presque certaine.

En raison de l'extrême étroitesse du rétrécissement, je dus employer en commençant des bougies très fines et légèrement coniques. Une fois introduites dans l'obstacle, elles y étaient serrées avec beaucoup de force : aussi nos progrès furent-ils peu rapides. Après vingt jours de traitement, à peine pouvions-nous introduire une bougie cylindrique de trois millimètres et demi. Déjà j'avais pu constater, outre le premier rétrécissement situé à neuf centimètres, un second à douze centimètres et demi. Huit jours après, la dilatation fut portée à quatre millimètres. Là, nous fûmes arrêtés par une grande résistance, opposée principalement par le premier rétrécissement. Pour la vaincre sans violence, je fis usage des bougies métalliques espacées par douzième de millimètre, et je laissai souvent deux ou trois jours d'intervalle entre nos séances. Un peu plus tard, les difficultés étant moins grandes, je repris la progression par sixième de millimètre. Mais toutes les fois que je remarquais des symptômes d'irritation, j'avais grand soin de laisser reposer le malade, convaincu qu'en pareil cas l'introduction des bougies n'aurait eu d'autre effet que de nous retarder encore.

Enfin, après deux mois et demi de traitement, nous arrivâmes à une dilatation de dix millimètres. Je crus devoir la pousser aussi loin, d'abord parce que la conformation du malade s'y prêtait, puis aussi parce que, dans les points qui correspondaient aux deux rétrécissements, je rencontrais toujours une virole très dense.

En résumé, M. B. avait fort peu souffert. Jamais, par

suite de notre traitement, il n'avait été forcé de garder la chambre, même une demi-journée. Il urinait avec la plus grande liberté, mieux, disait-il, qu'il ne l'avait jamais fait. Il s'introduisait facilement une bougie de dix millimètres.

Pour compléter et confirmer sa guérison, je lui recommandai très expressément de pratiquer cette opération au moins une fois par mois, dans les premiers temps; et c'est probablement au soin avec lequel il a suivi mes conseils qu'il doit d'être depuis trois ans à l'abri de toute récidive.

Longtemps encore après notre traitement, ce malade se plaignait d'éprouver toujours, quoique moins vive, cette sensation douloureuse dans la région du périnée qui était survenue après la première cautérisation. Enfin elle a complètement cessé, et le moyen qui m'a paru le plus efficace pour la combattre fut de prescrire matin et soir un quart de lavement froid.

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Il me serait facile de transcrire ici un grand nombre d'observations plus ou moins analogues à celles qui précédent, mais cela me paraît inutile. Je me suis borné à rapporter ce que l'expérience m'a appris; c'est à mes confrères, pour lesquels seuls j'écris, d'apprécier, d'après leur propre pratique, si j'ai bien vu. De leur assentiment résultera, je l'espère, la démonstration à laquelle j'attache le plus de prix.

Rappelons-nous maintenant le principe que nous avons établi. Le traitement des rétrécissements de

l'urètre doit avoir pour but, avons-nous dit, d'arriver graduellement, avec sûreté et peu de douleur, à une dilatation qui sera consolidée par l'introduction des bougies à des intervalles de plus en plus éloignés ; cette dernière précaution permettant seule de compter sur une guérison durable. Il restera maintenant à examiner les deux propositions suivantes , qui résument tout ce que j'ai avancé jusqu'ici : La méthode que je conseille est-elle applicable au plus grand nombre des cas? Offre-t-elle les avantages que je lui ai attribués ?

Relativement à la première question , je me bornerai à faire observer que le succès dépend surtout de la multiplicité des bougies et de leur graduation régulière ; que l'habileté manuelle du chirurgien l'accélère et le facilite. S'il se rencontre des exceptions, à mesure que le temps fera justice des faux systèmes et éteindra leurs fâcheuses conséquences , elles deviendront de plus en plus rares.

Quant à la seconde proposition, l'objection principale sera tirée de la durée du traitement; mais , remarquons-le bien, cette lenteur n'est qu'apparente.

Le chirurgien n'est nullement astreint à parcourir invariablement toutes les divisions de la filière.

Lorsque les circonstances le permettront , il franchira plusieurs degrés de l'échelle. En outre, il n'éprouvera point dans sa marche les interruptions si fréquemment occasionnées par l'inflammation que fait naître le séjour des bougies : aussi ne suis-je pas convaincu que la maladie exige pour sa guérison un

plus grand nombre de jours dans la nouvelle méthode que dans l'ancienne. Mais en admettant cette hypothèse, la question de temps ne serait nullement décidée.

En effet, chaque séance se trouve réduite d'une heure ou deux à quelques minutes. Certes, il serait puéril d'additionner de part et d'autre la somme des heures consacrées au traitement, et de tirer de là un argument en faveur de mes opinions. Mais n'est-ce pas le dérangement apporté dans ses habitudes, l'impossibilité de vaquer librement à ses affaires, qui font redouter au malade un long traitement ? Or, ici ces inconvénients disparaissent ; l'observation des simples règles d'hygiène est ordinairement suffisante.

A ce sujet, pendant longtemps mon étonnement fut grand. Surpris de ne rencontrer presque jamais d'inflammations, je cherchai l'explication de ce fait dans la nature particulière des individus que je soignais ; mais, à mesure que mes observations se sont multipliées, j'ai dû me rendre à l'évidence, et reconnaître là un des effets de la méthode que je suis.

Enfin je ferai valoir un dernier argument auquel j'attache une grande importance. Quelque traitement que l'on ait adopté, si le chirurgien n'enjoint pas au malade de maintenir la guérison par des soins convenables, ou bien si ses conseils ne sont pas suivis, il est probable qu'après un temps variable la maladie se reformera. Je sais combien est vaste et compliquée la question des récidives. Elle ne peut être résolue que par des chiffres recueillis après de longues pé-

riodes; car chaque système, j'en suis convaincu, fournirait, en nombres variables, d'heureuses exceptions. J'ai donné des soins à des malades affectés de rétrécissements fort graves dont la guérison fut rapide. Soit par incurie, soit que la simplicité d'un premier traitement leur eût laissé peu d'appréhension pour un second, plusieurs années s'écoulèrent sans qu'ils prissent les précautions, pour ainsi dire hygiéniques, que je leur avais très instamment recommandées; et cependant, au bout de ce temps, ils ne ressentaient encore aucune atteinte de leur ancienne maladie. Des exemples de ce genre, je le répète, seront toujours pour moi exceptionnels, et ne sauraient, quoique nombreux, ébranler ma conviction.

Mais, si la reproduction du rétrécissement est un fait commun à tous les procédés chirurgicaux, quelle différence dans les résultats!

Voici deux malades qui, au bout d'un même laps de temps, viennent de nouveau réclamer les secours de l'art. Leur position est identique en apparence: vous constatez de part et d'autre un rétrécissement de même diamètre.

Cependant vous apprenez que le premier a été traité par la dilatation, tandis que le second a subi diverses opérations; par exemple, qu'il a été cautérisé avec persévérance. Lorsqu'ils vous interrogeront sur la durée probable de vos soins, fixerez-vous approximativement à chacun une limite semblable? Certes, l'erreur serait grande.

Si, du moins quoique bien plus lentement obtenue,

dans le second cas , la guérison devait être semblable de part et d'autre ; mais ce n'est pas impunément qu'on aura favorisé la formation dans l'urètre du tissu de cicatrice.

Lors même que vous aurez obtenu une dilatation considérable, le second malade ne sera peut-être pas encore délivré de tous ses maux ; il se plaindra de douleur , de pesanteur dans la région du périnée; trop heureux, souvent, s'il voit disparaître , à force de soins et de patience, ce que j'appellerais plus volontiers une infirmité qu'une maladie !

Ainsi donc, eu égard à la fréquence et à la gravité variable des récidives , le meilleur traitement sera celui qui *modifia la moins la vitalité de l'urètre.*

J'emploie ici à dessein cette expression un peu vague, qui, entre les mains des cautérisateurs , a joué longtemps un grand rôle. Comment ont-ils pu faire prévaloir leur système , frappé qu'il était de réprobation par les esprits vraiment chirurgicaux de l'époque? je l'ignore. Mais ce qui me paraît plus bizarre encore, c'est la singularité de leur argumentation. Quand on leur demandait : Pourquoi cautérissez-vous? Accepter franchement la discussion et s'attacher à prouver comment la cautérisation , mieux que la dilatation , faisait disparaître les symptômes de la maladie, c'était s'avouer vaincus. Aussi, à toutes les questions , ils répondaient que le nitrate d'argent *modifie la vitalité.* Or, c'est pour cela précisément que l'emploi de cet agent me paraît exclusivement préjudiciable.

Malgré tous les avantages attribués originellement à la cautérisation , la plupart des chirurgiens reconnurent enfin que la méthode de Ducamp , très ingénieuse sans doute , ne répondait pas aux exigences de la pratique. On essaya alors d'inciser les rétrécissements ; on crut de nouveau avoir trouvé la solution définitive du problème , et la scarification fut prônée avec le même enthousiasme qu'avait inspiré jadis la cautérisation. Nous fûmes loin de partager ces espérances , qu'expliquait seulement à nos yeux le besoin de trouver un remède efficace contre une maladie aussi rebelle.

En effet , si l'on analyse la manière d'agir des scarifications , on voit qu'elles ne peuvent concourir à la dilatation de la partie rétrécie qu'à une condition : c'est que les lèvres de l'incision seront tenues écartées pendant tout le temps nécessaire à la cicatrisation. Sans cette précaution , la plaie produite par le scarificateur se réunira par première intention ou bien après avoir suppurré pendant quelques jours , et le rétrécissement reparaîtra.

Remarquons en outre que la présence d'un corps dilatant dans l'urètre est nécessaire pendant long-temps encore , alors même que la cicatrisation sera complète , car il faut s'opposer à la rétraction du tissu inodulaire qui sépare les deux bords de l'incision. N'est-ce pas ce que nous voyons journellement se passer sous nos yeux lorsque l'on pratique la section des brides ou des cicatrices vicieuses , lorsque l'on cherche à remédier au rétrécissement spon-

tané ou accidentel des orifices naturels, tels que la bouche, les narines, l'anus, etc.? Ne sait-on pas qu'un simple débridement n'aurait aucune chance de succès?

Mais si l'interposition d'un corps étranger entre les lèvres de la plaie, après l'incision, est un précepte absolu, ce moyen, hâtons-nous de le dire, échoue souvent malgré tous les soins imaginables. C'est là ce qui a engagé les chirurgiens à modifier à l'infini les procédés opératoires proposés pour combattre les difformités dont nous parlons; c'est là ce qui a donné naissance aux méthodes très ingénieuses de M. Dieffenbach et de M. Jobert.

Enfin, pour citer un exemple qui offre avec les rétrécissements de l'urètre la plus frappante analogie, l'incision des rétrécissements valvulaires du rectum n'échoue-t-elle pas constamment? Dans ce cas cependant il est facile de conduire avec une extrême précision l'instrument qui divise le cercle rétréci, de donner aux débridements une étendue convenable, de placer à demeure des corps dilatants, etc.

Si donc la reproduction du rétrécissement du rectum, à la suite du traitement par l'incision, est la règle (et nous ne craignons pas d'affirmer qu'une observation attentive justifie cette proposition), comment pourrait-il en être autrement pour les rétrécissements de l'urètre, qui opposent à l'application de la même méthode des difficultés beaucoup plus grandes? Est-il toujours facile de savoir en pareil cas ce que l'on incise? Lorsque le rétrécissement n'oc-

cupera point toute la circonférence du canal , l'opérateur pourra-t-il toujours avec certitude agir sur les tissus altérés et respecter ceux qui ne le sont pas ? Donnera-t-il à l'incision une étendue et une profondeur convenables ? Les douleurs , le spasme de l'urètre , ne s'opposent-ils pas souvent , après l'opération , à l'introduction des bougies dans le canal ?

Nous nous sommes borné à apprécier d'une manière générale les chances de succès du traitement par les incisions ; mais comme de cette discussion il résulte pour nous qu'il doit être abandonné , il serait superflu d'exposer longuement les accidents qu'il peut produire.

Nul doute qu'un grand nombre de malades ne guérissent après avoir été scarifiés ; mais la scarification , remarquons-le bien , est toujours unie à la dilatation. A celle-ci , selon nous , appartient le succès ; l'autre est dans le traitement un temps inutile et dangereux.

Pour démontrer plus complètement encore les avantages de la méthode que j'ai exposée , il conviendrait peut-être d'établir un parallèle entre elle et les autres systèmes de traitement ; d'énumérer les souffrances qu'ils imposent aux malades , contraints souvent de s'aliter , et les divers accidents qui entravent la guérison. Mais les médecins suppléeront facilement à cette lacune. Quant aux personnes étrangères à la science , il ne m'appartient pas de guider leur choix et de les préserver d'erreurs que je déplore.

Interrogée avec soin, l'anatomie pathologique conduit aux mêmes conclusions que la thérapeutique.

Pour examiner, après la mort, l'urètre d'un individu atteint de retrécissement, voici en général comment on procède. Après avoir reconnu avec une sonde l'obstacle déjà constaté pendant la vie, on détermine sa position : puis on incise dans toute sa longueur la paroi supérieure du conduit. Les personnes habituées à ce genre de recherches savent que souvent, au premier aspect, le retrécissement a disparu ; on croirait avoir sous les yeux un urètre sain. Mais si, se reportant au point qui devait être le siège de la maladie, on l'étudie plus attentivement ; si, par exemple, saisissant avec des pinces, au niveau du retrécissement, les deux bords du canal incisé, on exerce une traction transversale, on n'obtient aucune espèce d'allongement. Au contraire, dans les parties saines, l'extension est facile et considérable : c'est que le tissu des retrécissements est, par sa nature, un des plus dépourvus d'élasticité qui se rencontrent dans l'économie.

Contre cette vérité pratique sont venues s'annihiler bien des conceptions aussi ingénieuses que séduisantes. Naturellement on est assez porté à se représenter un retrécissement comme une virole élastique. Partant de ce principe absolument faux, on s'imagine qu'on obtiendra un agrandissement de diamètre en exerçant une traction méthodique dirigée du centre à la circonférence. Enfin, pour com-

pléter ce raisonnement, on maintiendra la dilatation assez longtemps pour vaincre la prétendue élasticité et rendre durable l'effet obtenu.

Quand je commençai l'étude des rétrécissements de l'urètre, j'ai payé mon tribut à ce genre d'erreur, d'autant moins excusable que j'étais parfaitement édifié sur la partie anatomique du problème. Assurément les tissus que j'avais si souvent observés n'étaient point de nature à se prêter à une dilatation immédiate, quelque rationnelle qu'elle pût être au point de vue mécanique, et cependant c'est vers ce dernier résultat que je dirigeai d'abord mes recherches.

Malheureusement, dans les sciences médicales, soit en raison de leur complication, soit imperfection de notre esprit, il en est souvent ainsi. Rarement la théorie seule décide *à priori* une question pratique, et son rôle se réduira peut-être longtemps encore à justifier les solutions trouvées par l'observation.

## RÉSUMÉ.

1° La guérison radicale des rétrécissements de l'urètre s'entend de deux manières :

L'une absolue, dans laquelle on suppose que, le traitement une fois terminé, le malade est exempt à tout jamais, non seulement des accidents, mais des plus légers soins relatifs à son affection. Cette signification ne peut être adoptée que par le vulgaire.

Dans l'autre signification pratique et médicale, le mot guérison exprime la cessation complète de tous les symptômes de la maladie, avec la faculté d'en prévenir le retour au moyen de quelques soins hygiéniques. C'est le sens médical qu'il convient d'adopter; l'autre est chimérique.

2<sup>e</sup> La dilatation méthodique, telle que nous la proposons, résout très simplement le problème dans sa véritable acceptation.

3<sup>e</sup> Les douleurs qu'elle impose au malade sont pour ainsi nulles.

4<sup>e</sup> Pendant la durée du traitement, le malade peut vaquer à ses affaires, sans être exposé à aucun accident inflammatoire direct ou métastatique.

5<sup>e</sup> La durée du traitement est d'un mois à six semaines ; chaque séance n'exige qu'une ou deux minutes.

6<sup>e</sup> Les récidives, dues constamment à l'incurie des malades, sont peut-être moins fréquentes, mais toujours infinitémoins graves que lorsque l'on a pratiqué diverses opérations laissant après elles des cicatrices dans l'urètre.



CORRESPONDENCE BETWEEN  
DR. SIMPSON, DR. RAMSBOTHAM,  
AND DR. LEE,  
IN RELATION TO UTERINE HÆMORRHAGE  
FROM PLACENTAL PRESENTATION.

*Note by Dr. Simpson.*

IT has already been shown (see GAZETTE for October 10) that Dr. Lee, while anxious to criticise others for statistical mistakes regarding placental presentations, has himself committed and published no small variety of numerical errors respecting the limited portion of placental cases that have occurred in *his own* practice. Thus, *first*, in his Clinical Midwifery, he states his recorded cases as 35 instead of 36; *secondly*, in one part of his "Lectures," he states the cases there tabulated as 36 instead of 38; *thirdly*, in the MEDICAL GAZETTE for Sept. 19, the sum total of his placental cases should be 44 instead of 43; *fourthly*, in the GAZETTE of last week, the ultimate case should be numbered 46, instead of 45; *fifthly*, all the eight numbers attached to his table of eight cases, in the GAZETTE for Sept. 19, are individually wrong; *sixthly*, among his placental cases, in his Clinical Midwifery, he has omitted any case under No. 290; *seventhly*, out of exactly the same 36 cases, he makes in his Clinical Midwifery 11, and in his Lectures 13, be complicated with rigidity of the os uteri, &c.

These errors, however, are trivial in degree compared with a strange and unfortunate statistical mistake which Dr. Lee has committed in the last week's GAZETTE.\* He has there published the results of 89 cases of placental presenta-

tion that have occurred in the practice of the respected Dr. Merriman. In a table which Dr. Lee gives of these cases, he makes Dr. Merriman *lose* 67 mothers out of the 89, or about 3 in 4; and save 22, or 1 in 4. The opposite, however, must evidently be the truth—that 22 of the mothers died, or 1 in 4; while 67 recovered, or 3 in 4.\*

These and the various other numerical inaccuracies which Dr. Lee is well known to have published in regard to the placental cases of *other* authors, will, it is hoped, render him in future a more charitable critic on this subject, seeing he is himself so *very* far from being infallible.

No. 1.—*Letter from Dr. Simpson to Dr. Ramsbotham.*

Stafford House, London, Sept. 20, 1845.

My dear sir,—I have had sent after me to London a collection of letters which had come to Edinburgh after my departure. Amongst them I find one† from you in reference to the report which I have given (in the March Number of the Edinburgh Monthly Journal) of the maternal mortality amongst the patients of the Royal Maternal Charity in cases of unavoidable hæmorrhage. I hasten to assure you that it gives me much pain to think that any unintentional mistake of mine should have

\* See Correspondence, p. 1080. The error was in Dr. Merriman's manuscript, from which the table was printed.—ED. GAZ.

† The tenor of Dr. Ramsbotham's letter was of the same nature as that of his two notes to Dr. Lee, published (p. 896) in the MEDICAL GAZETTE for 19th September; and, in the same way, stated the number of maternal deaths at the Maternity Charity from placental presentations to be 15 only out of 50 cases; while the number of children lost was 32. He mentioned, also, as in his notes to Dr. Lee, that the 44 cases of unavoidable hæmorrhage spoken of in his work on Obstetric Medicine, were all cases of *partial* presentation of the placenta.

To save repetition, Dr. Ramsbotham's first letter is not given. The letter bearing upon the subject will be found at p. 12.

given you a moment's annoyance, as nothing could be further from my wishes than to do so.

The paper you refer to is a statistical one, requiring no small degree of labour on my part; and I was at last obliged to print it amidst all the hurry and turmoil of a busy academic session. You are well aware of the almost insuperable difficulty of securing perfect accuracy in such tabular returns. I had occasion, some time ago, to point out to yourself, when writing you on the subject, an error of some importance in your own printed returns of placental cases belonging to the Maternity Charity. To ensure as great accuracy as possible in the various tables which I published in the memoir referred to, all the data in them were gone over more than once, and I was assisted by Dr. Keith and other friends in revising them; and the results, as they now stand, give no adequate idea of the labour expended in arriving at these results. For instance, in order to obtain as many data as possible from the fifty cases of placental presentation which had been recorded by you as occurring in the practice of the Maternity Charity, I searched over all the different volumes of the MEDICAL GAZETTE, in which your reports are contained, and entered each individual case by itself in a table shewing, under different headings, the number of the pregnancy, the date at which labour supervened, &c. &c. &c. Two contiguous columns contained the entries of the "results to the child," and the "results to the mother." In summing up the fifty resulting lines containing your fifty different cases, in order to get the single line to be found in the table which you complain of, I had either inadvertently entered, or subsequently copied the results to the child for the results to the mother, and the reverse. If the "results to the child" had been published in the first part of

the Memoir, I should at once have seen the error, and corrected it; but unfortunately I held the results to the child, in yours and other cases, in reserve for future publication.

Before leaving Scotland, I had nearly passed through the press, and had ready for separate publication, a long Memoir on the whole subject of the treatment of unavoidable haemorrhage by the extraction of the placenta before the child. In that Essay you will find the error I had made on your returns acknowledged and rectified, though (if I recollect properly) I still make out the number of maternal deaths to be different from you. However, a special reference to each fatal case is given.

You state in your letter that the 44 cases of placental presentation which I have appended to your name in the table were all cases of *partial placenta prævia*. I am quite aware of it. The table was made to shew the maternal mortality in *all* varieties of the complication, and under *all* modes of treatment—whether the membranes merely were ruptured, or the child turned, or the placenta spontaneously expelled, or the mother sunk without aid of any kind. If I had rejected the partial placental cases, such as your 44, because they were not so fatal as the complete, or the cases in which the liquor amnii was artificially evacuated, or the placenta spontaneously separated and expelled, because they were by no means so fatal as the turning cases, I might have easily brought out a result apparently stronger, but it would have been at the expense of strict and truthful accuracy. The proportion of maternal deaths under turning in placenta prævia, and where the presentation was generally central, forms, you will find, a subject of inquiry in a later part of the Essay. I hope to be able to send you a copy of the Essay itself in a few days after I return to Edinburgh.

In conclusion, allow me to state, that in lecturing and otherwise, I have repeatedly held up your returns of the practice of the Maternity Charity as the most remarkable in our professional annals, both for their intrinsic excellence, and for the very great practical success which they display in the treatment of the patients belonging to the Institution.

And believe me, my dear sir,  
Very faithfully yours,

J. Y. SIMPSON.

To Dr. Ramsbotham.

[*Addendum.*.—I have omitted to state, in the preceding letter, the way in which I was led to estimate the number of deaths among the infants in Dr. Ramsbotham's placental presentations as 33 instead of 32. (See MEDICAL GAZETTE for Sept. 19, p. 893). In Dr. Ramsbotham's Reports from 1829 to 1843, the number of infantile deaths in unavoidable hæmorrhage amounts to 31. The return for each of these years in the GAZETTE explicitly mentions the precise number lost in placental presentations. It is not so in the report for 1828 (MED. GAZ. Vol. III.). During that year 4 children are returned as born dead after severe uterine hæmorrhage, without its being specified whether these deaths were in connection with unavoidable or accidental floodings. As I found 3 cases of unavoidable, and 3 cases of accidental hæmorrhage in the list for that year, I divided, in want of more precise information, the number of infantile deaths equally between them, two to each, and hence made the whole number in placental presentations amount to  $31 + 2$  or 33. Dr. Ramsbotham, however, assures me, that among the three unavoidable hæmorrhages of that year only 1 child was lost; while in the 3 accidental hæmorrhages all the children were born dead.]

No. 2.—From Dr. Simpson to Dr. Ramsbotham.

My dear sir,—As soon as I received your letter last week, I wrote off to Edinburgh to have sent up to me here the original long manuscript table that had been constructed from your reports of the placental cases at the Maternity Charity, published in different volumes of the MEDICAL GAZETTE, and that formed the basis of my calculations regarding them.

My assistant and friend, Dr. Keith, tells me, in a note which I have this morning received from him, that he cannot lay his hands upon the Table, amidst the present state of confusion of my books and papers. The fact is, I lately went into a new house in Edinburgh, and the workmen and painters are still so busy with it, that everything is as yet entirely out of its place. Dr. Keith, however, has furnished me with some loose notes, which partly supply the defect. These notes confirm the impression which I mentioned to you in my last letter, that you were somewhat in error in your own account of your own placental cases.

The period of the reports of the Maternity Charity, to which your calculations and mine equally refer, are from 1828 to 1843, both years included.

In your excellent work on Obstetric Medicine, you twice state (see p. 725, and again p. 726), and found calculations upon the number of placental presentations occurring during the above period, as amounting to *Forty*. At p. 721, and in your later letter to me, you calculate them as *fifty* in number. Their actual number (I believe you will find) to be *fifty-one*, viz., III. in the year 1828; I. in 1829; II. in 1830; III. in 1831; III. in 1832; II. in 1833; I. in 1834; III. in 1835; VIII. in 1836; III. in 1837; I. in 1838; V. in 1839; X. in 1840; I. in 1841; IV. in 1842; and I. in 1843.

In your letter to me, you state the

number of maternal deaths in these placental cases as *fifteen*: I find *sixteen* deaths among the mothers in the cases mentioned in your published reports, viz. I. in the report for 1829; I. in 1830; I. in 1832; I. in 1833; I. in 1836; I. in 1837; III. in 1839; IV. in 1840; II. in 1842; and I. in 1843.

I have been thus particular in noting the years, because, as you are aware, it is not always very easy to discover the precise results of the placental cases, from the form in which you have arranged and published your reports in the GAZETTE. And if, in comparing the above list with yours, you detect any error, however slight, I shall, I sincerely assure you, feel very greatly obliged by your pointing it out to me, and be delighted to correct my tables accordingly, my great object of course being as great a degree of accuracy in them as possible.

I do not think that the returns of Lying-in Hospitals afford us by any means such correct estimates of the frequency and fatality of placental presentations, as of other obstetric complications: for women do not seek, and are generally not admitted into them, but at or near the full time, while the most common, and also certainly the most dangerous cases of unavoidable haemorrhage, are found occurring from the sixth to the ninth month. Only two, for instance, of Dr. Collins' eleven hospital cases were premature; the other nine were at the full term. Hence the value, in one respect, of returns like those of the Maternity Charity.

I was very sorry I missed seeing you to-day when you called. Professor Liebig was here at the time.

Believe me, my dear sir,

Very faithfully yours,

J. Y. SIMPSON.

To Dr. Ramsbotham.

Stafford House, London,  
26th Sept., 1845.

No. 3.—*From Dr. Ramsbotham to Dr. Simpson.*

My dear sir,—I thank you much for your letter of yesterday. I had already detected an omission of one case of placental presentation among my published reports, having looked carefully over both the original tables and the summary in the GAZETTE since I wrote you to Edinburgh. I could not at first account for it, but now it has become quite clear to me.

For the sake of greater accuracy, and less chance of error, as I thought, I compiled the tables published in the GAZETTE for August 9th, of last year, from my *manuscript*, instead of taking them from the published yearly statement; and I find that in the year 1837, one of the shoulder presentations, which was also complicated with *placenta prævia*, had been overlooked, as regarded the latter irregularity, by me, owing to the manner in which I had entered the case in my column for "Remarks;" so that I left it out, introducing it only as a transverse presentation. Thus my *extra* anxiety for correctness in respect to those tables from which I drew my calculations, led me into a mistake. One death I *purposely* omitted,—and it occurs in the same year,—because, although the patient had been delivered under a placental presentation, that irregularity could have had nothing to do with her death. She recovered perfectly, had been for many days engaged in domestic affairs, though she had never left her house; our superintendence of her had ceased; and on the day month from her delivery she was seized with a fit of apoplexy or convulsions, while in a violent passion, and died almost immediately. From this history you will agree with me, I think, that I am fully warranted in omitting the case as one of death *consequent* upon placental presentation in my synopsis. I have no doubt this woman would have been

restored to health, had it not been for this unfortunate intemperance.

We have already had a correspondence, you may recollect, about the *forty* instead of *fifty*. Both in the GAZETTE, and the volume, in the *table* the number stands as 50, while in the calculations founded on the tables 40 is printed instead. Indeed the fault was mine, or a friend's who copied for me; for the calculations are deduced from 40, and not 50; shewing that it is not an error of the press, but one of transcription.

Believe me, my dear sir,

Very faithfully yours,

FRANCIS H. RAMSBOTHAM.

14, New Broad Street,  
Sept. 26th, 1845.

No. 4.—*Dr. Simpson to Dr. Ramsbotham.*

My dear sir,—For your last note I return you my best thanks. On Saturday I went down into Berkshire, or you would have heard from me sooner.

I am happy to think that you are convinced that my summing up of your cases of placental presentations is so far more correct than your own, notwithstanding the very great pains you have manifested upon their calculations. If I was anxious to adduce any evidence of the care I had bestowed on the inquiry, I could not desire a stronger testimonial.

In page 725 of your work, the number of placental presentations should stand as 1 in 700·8, instead of 1 in 893·5; and at page 726, the number of children lost in placental presentations should stand as 62·3 per cent., instead of 80·0 per cent.

You will find in my Essay, when published, one or two slight corrections in the tables; and I am sure you will readily grant me, that seeing the difficulty of making out accurately, and at first, *one* set of cases like your own, I may be excused if, in summing up various sets of them, I fell into one or

two trivial errors with Smellie and others, whose cases were scattered, and difficult to collect and compare.

I have seen Dr. Lee's paper in the MEDICAL GAZETTE of the 19th, to which your note a few days ago referred me, and have been both grieved to witness its improper tone, and at the same time amused by its odd mistakes. It is confessedly difficult to avoid arithmetical blunders, but surely it is possible to avoid such errors in regard to history and matters of fact, as he has committed. I know not how I have happened to excite so much of his displeasure. He and I used to be good friends. In lecturing, I have always maintained that English midwifery was much indebted to him for pressing upon our notice, and so far confirming, some of the *pathological* views of our continental neighbours, regarding phlebitis, and fibrous tumours of the uterus. It is true that I have, at the same time, constantly looked with distrust upon his anatomical labours, because there he ventured on a field that was quite alien to his other pursuits; and I have ever doubted whether his supposed anatomical discoveries would ultimately prove anything more than mere anatomical errors. His two highest attempts in this direction, have been his paper on the Structure of the Placenta, and his later Essay on the Nerves of the Uterus during Pregnancy,—both published in the Philosophical Transactions. The first of these papers was published about 1832, and Dr. Lee himself has lately admitted, that a year or two afterwards, he found that his alleged discovery regarding the placenta, was a pure anatomical mistake. For my own part, I believe he will betimes confess the same regarding the uterine nerves. I know well he brought forward some time ago, from various anatomists and physicians, written evidence of the strongest kind, in fa-

vour of his views. It was to my mind suspiciously strong. Professor Tiedemann published a description and engravings of the Nerves of the Pregnant Uterus about 1822. Dr. Lee began his investigations in 1837 or 1838, or nearly sixteen years afterwards; yet one of the testimonials published by Dr. Lee in support of his anatomical views was so exceedingly determined as to certify against the very progress of time itself, and suggested that Dr. Lee's investigations were anterior to Professor Tiedemann's. See the good-natured testimonial given by Dr. John Davy to Dr. Lee, and printed with the others in the *GAZETTE*. You are probably aware that Mr. Beck here, has more lately and elaborately pursued the same inquiry into the uterine nerves, having begun it, I believe, to prove Dr. Lee's dissections right, and ending by demonstrating them to be wrong. Some of the gentlemen that certified for Dr. Lee, are now, I am told, quite convinced that Mr. Beck is correct, and Dr. Lee the reverse. Drs. Todd, Sharpey, Owen, Quain, Bowman, and other distinguished anatomists, who have examined Mr. Beck's beautiful dissections, are, I have been credibly informed, no longer in any doubt whatever, that Dr. Lee is altogether in error on the whole subject, and has uselessly thrown away much valuable time on the inquiry. Surely, my conscientious and confessed belief of all this, however, and my trust, with others, in the perfect accuracy of Mr. Beck, is no reason for having recourse to the remarks of those which compose the latter part of Dr. Lee's communication to the *GAZETTE*.

After reading Dr. Lee's observations in the *GAZETTE*, I wrote him a private note, in which (I hope) I used no expression of recrimination, and asked him if he would kindly point out to me any other arithmetical inaccuracies he might have observed in my paper. I

received in answer no reply to my request, but he sent me a perverted statement of Portal's cases that had nothing to do with the matter. I knew that he was intimate with Portal's work, and was certainly more than surprised by his deliberate and uncalled for misstatement regarding that author's cases. It must pain every friend of Dr. Lee's and of the profession to see him following such conduct. I am sorry to add, that in another note, he insisted upon repeating his misstatement to me. Of course, I had no alternative but to decline any more of a correspondence in which such principles were betrayed.

Believe me, my dear sir,  
Very faithfully yours,

J. Y. SIMPSON.

*To Dr. Ramsbotham.*

Stafford House, Sept. 1845.

#### No. 5.—*From Dr. Simpson to Dr. Lee.*

My dear sir,—A few days ago I received from Dr. Ramsbotham a letter, stating that you had directed his attention to some arithmetical inaccuracies in an *Essay* of mine, published in Dr. Cormack's *Journal* for last March.

I inclose you a copy of my answer to Dr. Ramsbotham's very kind and gentlemanly letter.

As I am publishing the same *Essay* in a separate and extended form, and am very anxious it should be as free from arithmetical errors as possible, I shall feel deeply obliged by your mentioning, or noting down for me, any other errors that you may have detected among the figures.

Any one who has worked at medical statistics, of the kind included in the *Essay* in question, knows how difficult, or indeed almost impossible, it is to go through all the different steps of extracting the required data, making the calculations from these data, arranging and copying the calculations when made, and finally getting them through

the hands of the printer, without some errors. And certainly there is no department of authorship in which one stands more in need of, and ought to feel more grateful for, the kindly correction and assistance of his professional brethren, even though, as in Dr. Ramsbotham's case, they do not alter the resulting statistical fact, or alter it only in a fractional degree. Besides, the investigation is of such a kind, that two persons, with every anxiety for truth and accuracy, may read and interpret differently the very data upon which we have to work. Among Dr. Collins' returns, for instance, of his own practice in the Dublin Hospital, I make out one death *more\** from unavoidable hæmorrhage than he does. On the other hand, Dr. Churchill (see the article "Hæmorrhage," in his general work on Midwifery) has given a fuller list of cases and deaths from unavoidable hæmorrhage, in Smellie's practice, than I could find at the time in Smellie's Works; and yet he ultimately arrives at the same general statistical fact that I have done, namely, that taking a large series of cases, it will be found that *one out of every three mothers dies* under this presentation of the placenta. In the same way, while Dr. Churchill makes Giffard's recorded cases of unavoidable hæmorrhage amount to 29, I could find only reports of 24; and you state somewhere that he has about 20. Again, in your Clinical Midwifery, you state that Portal's Treatise contains an account of "eight" cases of uterine hæmorrhage from presentation of the placenta; whilst I make out (if I recollect properly) that he notices and describes the results of thirteen or fourteen cases, which he had himself met with in his own practice.

I have intended to look in upon you for some mornings past, but I find the

early part of the day generally dissipated here by various employments before I well know that the day has really begun.

Very truly yours,

J. Y. SIMPSON.

To Dr. Lee.

Stafford House, St. James's,  
23d Sept., 1845.

No. 6.—From Dr. Lee to Dr. Simpson.

4, Savile Row, 23d Sept., 1845.

Dear sir,—As you are so much engaged, I should be sorry that you should put yourself about by calling upon me. Respecting Portal's cases, it will be seen that, in my Clinical Midwifery, I refer to eight cases "in which he found the placenta not merely at the mouth of the womb, but adhering to the whole neck of the uterus,"—thus pointedly distinguishing them from the other cases of partial placental presentation related in his work, or referred to without the details being given.—I remain, dear sir,

Very faithfully yours,

R. LEE.

To Dr. Simpson.

No. 7.—From Dr. Simpson to Dr. Lee.

Dear sir,—I did not in my last note mention the mistake into which you had fallen, with regard to the exact number of Portal's cases, with any idea whatever of blaming you for an error of the kind. In fact, though you state in your Clinical Midwifery that "Portal's Treatise (1685) contains an account of eight cases of uterine hæmorrhage in which," &c.; you more correctly observe, in your newly published Lectures, that "Portal's Treatise (1685) contains the histories of eight or more cases of uterine hæmorrhage, in which he found, on introducing the hand to turn, that the placenta was not merely at the os uteri, but adhering to the cervix all round."

If you had at once allowed to me, that Portal had met with thirteen or

\* See Case No. 11, in his chapter on arm presentations.

fourteen instances of placental presentation, and that you were wrong in limiting the total number of his observed cases to eight, I am sure neither I, nor any one else, could have attached any the slightest blame to you; for we are all more or less liable to commit errors, and, as I formerly observed, are all anxious to correct them when pointed out.

But I must confess that I do most sincerely grieve and lament to observe the mode and means by which you attempt (in the note I have just received from you), to escape from the numerical error into which you had fallen. For I can assure you that Portal does *not* in his work relate or refer to one single instance of *partial* placental presentation. And I fear I must at the same time believe, that you know all his placental cases accurately, for you have published a lengthened and careful analysis of them in your printed lectures.

I have the honour to be,

Yours, &c.

To Dr. Lee. J. Y. SIMPSON.

Stafford House, 24th September, 1845.

#### No. 8.—Dr. Lee to Dr. Simpson.

4, Saville Row, 25th Sept. 1845.

Dear sir,—I am sorry to differ from you, but it is still my opinion that Portal's book contains the histories of eight cases of complete placental dresentation, and that all the others were cases of partial placental presentation.—I remain, dear Sir,

Very truly yours,

To Dr. Simpson. R. LEE.

#### No. 9.—From Dr. Simpson to Dr. Lee.

Dear sir,—The "eight" cases of placental presentation which Portal has detailed at full length, were all cases in which (to use again *your own* words) "the placenta was not merely at the os uteri, but adhering to the cervix all round." Hence, I take it you will grant that *they* were complete placental presentations; you tell me, however, in your note of the 23d, that in describing those eight cases in language like the above, you "thus pointedly distinguished them from the

other cases of *partial* placental presentation related in his work, or referred to without the details being given." I have procured here a sight of Portal's work, lest my memory should have possibly deceived me, and find that the other cases (six in number), are as follows. After relating Case 29, in which the head of the child, in its exit through the os uteri, actually perforated **THROUGH** the placenta itself, (the placental presentation being hence complete), Portal adds, that not long afterwards he "delivered a gentlewoman in St. Dennis Street, under the *same* circumstances in the presence of Dr. Linkard," &c. In Case 51, Portal tells us, that the placenta was "placed just before, and quite across the whole inner orifice of the uterus," and "in concluding (I now again quote *your own words*), the history of this case (51), he states, that in the year 1683 he had completed the delivery successfully in five *similar* cases, all the women having recovered." (Dr. Lee's Lectures, p. 366). "In the year 1683," observes Portal, in his own account, "I delivered five women under the *same* circumstances, &c."

Assuredly, there is not one hint or syllable regarding these "other" cases, or any one of them being *partial* placental presentations, but the reverse.

I feel again compelled to deplore deeply your thus venturing for a *second* time to resort to misstatement, in order to try to cover what was originally a very simple and very pardonable error, regarding the number of Portal's recorded cases. And you must really excuse me if I decline any farther correspondence on this subject.

I have the honour to be,

Your most obedient servant,

To Dr. Lee. J. Y. SIMPSON.

Stafford House, 27th October, 1845.

#### No. 10.—From Dr. Lee to Dr. Simpson.

4, Saville Row, 29th Sept., 1845.

Sir,—Noboby asked you to correspond with me respecting Portal's cases, or any thing else. You require, I perceive, a little of the discipline which was so efficacious in the case of your hoaxing friend.—I am, sir, yours, &c.

R. LEE.

To Dr. Simpson.

32  
*G.H.Y.* 24

# SOME REMARKS ON THE TREATMENT OF UNAVOIDABLE HÆMORRHAGE

EXTRACTION OF THE PLACENTA BEFORE THE CHILD.

WITH

*A few Observations on Dr. Lee's Objections to the Practice.*

BY J. Y. SIMPSON, M.D. F.R.S.E.

PROFESSOR OF MIDWIFERY IN THE UNIVERSITY OF EDINBURGH.

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*From the London Medical Gazette, October 10, 1845.*

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ALL the more severe forms of uterine hæmorrhage that are liable to occur in the later periods of pregnancy, and during delivery, are generally allowed, by obstetric pathologists, to depend upon the separation of a greater or less portion of the placenta from the interior of the uterus. When such a separation takes place, *two* surfaces are exposed, namely, *first*, a part of the inner surface of the uterus, and, *secondly*, the corresponding part of the outer, or maternal surface of the placenta. Both of these exposed surfaces present a number of open vascular orifices left by the laceration of the utero-placental vessels which formerly connected them. From which set of open vascular orifices—the uterine or the placental—does the resulting hæmorrhage principally proceed?

Most accoucheurs seem to believe that the blood effused in those hæmorrhages which occur before or during labour, comes from the exposed *uterine* orifices. "It is (observes Dr. Lee) from the great semi-

lunar, valvular-like, venous openings in the lining membrane of the uterus, which you have seen in various preparations, and of [from] the arteries which are laid open by the separation of the placenta, that the blood *alone* flows in uterine hæmorrhage."—(Lectures on Midwifery, p. 361.)

But arteries, particularly when they are so long and slender as the utero-placental arteries are, do not give rise to any marked degree of hæmorrhage when they are lacerated or torn through; and bleeding does not readily occur from the venous openings exposed on the interior of the uterus, because venous hæmorrhage by *retrogression* (which the blood escaping backward into the uterine cavity would be) is here prevented by a variety of anatomical and subsidiary means, which I have elsewhere taken occasion to describe at some length.

In the passage that I have quoted above from Dr. Lee's published Lectures, Dr. Lee does not allow that the blood, in uterine hæmorrhage, proceeds in any degree from

the open venous orifices existing on the surface of the separated portion of placenta, the discharge proceeding, in his opinion, from the exposed *uterine* surface "alone." But I know of no reason, anatomical or otherwise, for alleging that the open *placental* orifices do not bleed; and, on the contrary, I believe with Dr. Hamilton and others, that the discharge issues principally or entirely from the vascular openings which exist on that exposed placental surface. These placental orifices are not, like the uterine, surrounded by contractile fibres capable of constricting them; they are in free communication with the general vascular system of the mother through the medium of the maternal vascular, or cavernous system of the placenta; and the blood in that cavernous system escapes readily from the exposed venous orifices on the surface of the placenta—that being, in fact, so far, its natural and *forward* course.

In cases in which the placenta is partially and repeatedly detached before labour begins (as happens frequently in placental presentations), before each attendant attack of hæmorrhage is arrested, the vascular system of the separated portion of placenta seems to require to become blocked up and impervious, with coagulated and infiltrated blood. This obliteration of its vascular cells prevents the further circulation of maternal blood through the detached part of the organ, and hence prevents also the further escape of it from its exposed surface. Each new detachment gives rise to a renewed hæmorrhage, which again ceases on the sealing up of the vascular system of the detached part. A few cases of placental presentation are on record in which there was *no* attendant hæmorrhage when labour supervened, the tissue of the placenta having, throughout the whole organ, previously become so morbidly changed, obstructed, and impervious, as not to have any quantity of blood circulating in it and ready to escape, when at last its surface was separated from the interior of the cervix uteri under the occurrence of the uterine contractions.

In common cases of unavoidable hæmorrhage, the amount of the attendant flooding seems to be as much regulated by the quantity of placental surface *still* remaining attached to the uterus, as by the quantity *already* separated from it—the degree of flooding depending as much, or more, upon the extent of the means of supply of blood as upon the extent of its means of escape. And in proportion as we approach nearer and nearer a *total* separation of the placenta, the number of its *afferent* utero-placental vessels is diminished, till at last we find that when the one organ is once completely separated from the other, the flooding is instantly moderated, or entirely arrested; for the

placenta ceases to yield any discharge of maternal blood as soon as its own supplies from the maternal system are thus cut off by the dis severment of all its organic and vascular attachments with the uterus.

Some years ago, I happened to see two cases of unavoidable hæmorrhage, in which the placenta was spontaneously expelled for some hours, before the child itself was born. In both cases the attendant hæmorrhage moderated, or entirely ceased, as soon as the whole placenta was completely detached. These instances, and others with which I was previously acquainted, forcibly suggested to my mind the idea that, under some complications in unavoidable hæmorrhages, we might here (as in many other obstetric operations) adopt the principles of treatment at times successfully acted upon by nature herself, in her own unassisted management of such cases. I knew the fearful maternal mortality accompanying placental presentations, and that it was as great, or even greater, than the fatality among patients attacked with yellow fever, or subjected to lithotomy. In order to ascertain if the *total* and complete detachment of the placenta afforded a greater chance of life to the mother, I collected and published in Dr. Cormack's *Journal of Medical Science* for March last, notices, which at that date I had brought together, of 141 cases of placental presentation in which the placenta was expelled or extracted before the child. The deductions which I ventured to draw from an analysis of these 141 cases were to the following effect:—

1. The *complete* separation and expulsion of the placenta before the child, in cases of unavoidable hæmorrhage, is not so rare an occurrence as accoucheurs seem usually to believe; and it is not by any means so serious and dangerous as (according to the commonly received doctrines of uterine hæmorrhage) might *a priori* be expected.

2. In 19 out of 20 cases in which it has happened, the attendant hæmorrhage was either at once altogether arrested, or became so much diminished as not to be afterwards alarming.

3. The presence or absence of flooding after the complete separation of the placenta, does *not* seem in any degree to be regulated by the extent of the interval intervening between the detachment of the placenta and the birth of the child.

4. In 10 out of the 141 cases, or in 1 out of 14, the mother died after the complete expulsion or extraction of the placenta before the child; whilst, as we shall see immediately, about 1 in every 3 of the mothers dies under turning and extraction of the child in unavoidable hæmorrhage.

5. In 7 or 8 out of these 10 natural deaths, the fatal result seemed to have no

connection with the complete detachment of the placenta, or with consequences arising directly from it; and if we did admit the 3 remaining cases, (which are doubtful), as leading by this occurrence to a fatal termination, they would still only constitute a mortality from this complication of 3 in 141,—or of about 1 in 47 cases.

These facts tend strongly to shew that the artificial and complete detachment of the placenta would in all probability be in some cases and varieties, at least, of unavoidable haemorrhage, accompanied with much saving of maternal life. I know further, that in several instances recorded by Collins, Ramsbotham, Lowenhardt, &c. this treatment had been followed with success, when perchance it had been had recourse to by midwives, and others, under supposed mismanagement, and in ignorance and defiance of all the established rules of practice in this special complication.

I subjoin in a foot-note\* the details of a case of this description very kindly forwarded to me, some time since, by Mr. Cripps of Liverpool. I insert it as, at one and the same time, illustrative both of the preceding remark, and of some of the other observations which I have already offered.

Exactly a year ago, I had an opportunity of putting, for the first time, to the test of experience, the practice which the foregoing remarks all lead to suggest, of *detaching, and,*

\* "I was sent for—Mr. Cripps writes me—a few days ago, about 8 P. M., to see a poor woman who supposed herself to be at the early part of the last month of pregnancy with the third child. She had had occasional flooding to no great extent for a week previously. On the morning of the day on which I saw her, a surgeon had been sent for in consequence of the occurrence of several labour pains, together with a good deal of haemorrhage. This gentleman being out of town, his assistant went; he remained with her during the day, and in the evening, finding things not going on so favourably as he wished, he sent for a friend of his employer's, who, soon after his arrival, sent for me. On making an examination, I found an arm down, which was much swollen, and the pains very severe. I immediately gave one drachm of laudanum, and on their subsiding, turned without much difficulty. The funis was divided, only about four or five inches remaining, and appeared as though it had been cut. On expressing my surprise at this circumstance, I was informed that it was cut when the after-birth was taken away, about 10 in the morning. Not believing it possible that such could be the case, there having been no haemorrhage whatever from that hour until the period of delivery, I searched for the other portion of the navel-string, but not finding it, and being again assured that "the after-birth had come in the morning," I introduced my hand into the uterus, and made a most careful examination; it was contracting satisfactorily, but was perfectly empty. I watched her strictly until her complete recovery. I had every portion of discharge saved for my inspection, and am therefore perfectly satisfied that this is a case in which the placenta presented, and was removed 10 hours previously to the birth of the child, and that, in the meantime, there was no haemorrhage whatever."

*if necessary, extracting the placenta and not the child in unavoidable haemorrhage.* The lady (a patient of Mr. Hill of Portobello), was taken in labour between the 7th and 8th month of pregnancy, and, in consequence of the severity of the discharge, was blanched and prostrated when I first saw her. The vagina was filled with coagula, and the os uteri was, in consequence of its small size and great height, reached and passed with difficulty, so as to ascertain fully the presentation of the placenta. Anterior to it I was able after a short time to reach and rupture the membranes. Notwithstanding this, however, along with the exhibition of ergot, &c., the discharge and sinking continued to go on. It seemed very difficult and dangerous to attempt to turn in consequence of the state of the os, and as the edge of the after birth was offering to protrude through it, I separated and gradually extracted the whole placental mass. From the time that this was accomplished all haemorrhage ceased. The cord was cut, and the placenta removed from the bed. The infant came down slowly, and was safely expelled about two hours afterwards. The mother made a perfect and speedy recovery.

Similar cases of the successful adoption of the same practice have, since the period at which my paper appeared in Dr. Cormack's Journal, been published by Mr. Wilkinson, Mr. Greenhow, Mr. Jones, and Dr. Maclean. In all these instances the mothers were saved, and rapidly recovered. Dr. Lever and Dr. Bird have informed me within the last week, of two other recent successful instances of the same practice. In the course of a short time it seems not unreasonable to expect, that we may have a sufficient number of cases recorded, to enable us to judge with greater certainty and precision of the merits of this plan of treatment, and of the particular placental complications to which it may be specially applicable.

The proposal of the practice of separating and extracting the placenta before the child in unavoidable haemorrhage, and thus (to use the expressions of Dr. Robert Lee), "departing from the rule (of turning the child) which has been established in the treatment of cases of placental presentation for the last two hundred years," and "subverting the established rules of practice in the treatment of cases of such vital importance," has, as might naturally be expected, given rise to considerable discussion and difference of opinion. In the MEDICAL GAZETTE for September 19th, I find that Dr. Lee has entered his present dissent against the proposed treatment. The tone and character of Dr. Lee's remarks might save me from the necessity of offering any answer to them; but, for the sake of the practice under dispute, I shall correct in de-

tail some of the more prominent mistakes which his observations appear to me to contain.

*First.*—Dr. Lee appears to see no reason to depart from the practice which has been followed in placental presentations from the days of Ambrose Paré to the present time. The usual practice in these cases is well known to all. “The operation of turning, is (Dr. Lee observes), required in all cases of complete placental presentation,” but

“is not necessary in the greater number of cases in which the edge of the placenta passing into the membranes, can be distinctly felt passing through the os uteri,” (Lectures, p. 372). In these last, rupture of the membranes is sometimes sufficient.\* In his paper in the *GAZETTE*, Dr. Lee has given the following tabular view of eight late cases of placental presentation, in illustration of the success of the ordinary mode of treatment.

No.	Complete. or Partial.	Treatment.	Child.	Mother.
36	Complete.	Turning.	Dead.	Recovered.
37	Complete.	Turning.	Alive.	Recovered.
38	Partial.	Membranes ruptured.		Recovered.
39	Partial.	Craniotomy.	Dead.	Recovered.
40	Partial.	Craniotomy.	Dead.	Recovered.
41	Partial.	Craniotomy.	Dead.	Recovered.
42	Complete.	Turning.	Dead.	Recovered.
43	Uncertain.	Perforation of Placenta.	Dead.	Recovered.

If the above table afforded a correct idea of the success of the common practice in placental presentations, I should never have attempted to change it. But unfortunately, turning, “which is required (according to Dr. Lee) in all cases of complete placental presentation,” is followed in this complication with very fatal and disastrous

results. Among Dr. Ramsbotham’s reports of the Maternity Charity and Dr. Lee’s previously published cases, I find 61 instances in all reported, of placental presentations, in which turning and extraction of the child were had recourse to. The following table shows the results.

*A tabular view of the results of 61† cases of Turning in Placental Presentations.*

Reporters.	No. of Cases operated on.	No. of Mothers saved.	No. and proportion of Mothers lost under this treatment.
Dr. Lee . . .	24	14	10 or nearly 1 in every $2\frac{4}{5}$ .
Dr. Ramsbotham.	37	23	14 or nearly 1 in every $2\frac{7}{10}$ .
Total . . .	61	37	24 or nearly 1 in every $2\frac{1}{2}$ .

Hence, 24 out of the 61 mothers sunk under this treatment. More than 1 out of every 3 was lost. Or, in other words, under this practice about 65 per cent. of the mothers were saved, and 35 percent. of them died.

The great mortality resulting from the treatment of turning in placental presentation, may be more strongly shewn to some minds if the fact is stated in another form. In order to ascertain the fatality of the Cæsarean section abroad, Dr. Churchill collated with much care the histories, from foreign authorities, of 371 cases of the

operation. Out of these 371 cases, 217 mothers recovered, and 154 or nearly 1 in every  $2\frac{4}{5}$ , died. (Midwifery, p. 318.) This is exactly, and to a fraction, the degree of maternal mortality accompanying turning in placental presentations, in the cases reported by Dr. Lee in his Clinical Midwifery. *In other words, the success of turning in unavoidable hæmorrhage, in Dr. Lee’s private and consultation practice (as reported in that work) has not been greater than the reputed success of the Cæsarean section upon the continent of Europe.*

When we see that the results of turning the child in placental presentations are so

\* Some years ago the common practice of rupturing the membranes in partial placental presentations appears not to have been recommended by Dr. Lee. “It may be laid down (he states), as a rule admitting of no exception, that when hæmorrhage occurs from the placenta being situated over the os uteri, artificial delivery must be performed;” and he goes on to show it is performed by turning and extracting the child. (Researches on Diseases of Women, p. 207.)

† To prevent error, it may be proper to repeat that these 61 instances include *all* the cases of turning in placental presentation, which I find reported in the returns of Dr. Lee and Dr. Ramsbotham. Dr. Lee’s returns are those of his private and consultation practice. Dr. Ramsbotham’s returns are those of the practice in his own district of the Royal Maternity Charity.

very mortal in the hands of two such distinguished accoucheurs as Dr. Ramsbotham and Dr. Lee, what degree of success can we expect to follow it in the hands of the general mass of medical men?

Last year Dr. Lee most truly and justly remarked of turning in placental presentation, “*At best it is a dangerous operation*, and you can never tell with certainty whether or not the patient will recover after its performance, however easily it may have been effected.” (*Lectures*, p. 373.)

*Secondly*.—Dr. Lee seems to argue as if I recommended the artificial detachment of the placenta in *all* forms of placental presentation in which turning is at present adopted. On the contrary, I have explicitly mentioned it as a mode of treatment to be adopted when rupturing of the membranes is insufficient, and turning is either inapplicable or unusually dangerous. I believe it will be found, for instance, the proper line of practice in severe cases of unavoidable haemorrhage complicated with an os uteri so insufficiently dilated and undilatable as not to allow, with safety, of turning; in most primiparæ; in many of the cases in which placental presentations are (as very often happens) connected with premature labour and imperfect development of the cervix and os uteri; in labours supervening the earlier than the seventh month; when the

uterus is too contracted to allow of turning; when the pelvis or passages of the mother are organically contracted; in cases of such extreme exhaustion of the mother as forbid immediate turning or forced delivery; when the child is dead; and when it is premature and not viable.

As an illustration, I shall take the first set of cases I have adverted to: “There is not unfrequently (says Dr. Lee) most profuse and alarming flooding from complete placental presentation, where the os uteri is so thick, rigid, and undilatable, that it is impossible to introduce the hand into the uterus without producing certain mischief. In 13 (he adds) out of the 36\* cases contained in the following table, the os uteri was rigid and undilatable.” Hence, this complication occurred as frequently in Dr. Lee’s practice as in about one out of every three of his placental presentations. In his Clinical Midwifery, out of 35† cases alleged to be reported, in 11 there had been more or less rigidity of the os uteri with dangerous haemorrhage. From the mode in which the individual reports are drawn up, it is by no means easy to determine exactly and with perfect precision, the “eleven‡” cases which Dr. Lee himself classes under this remark, but I believe I have correctly given them in the following table:—

*Table of Eleven Cases of Placental Presentation, from Dr. Lee’s Clinical Midwifery: shewing the combination of “more or less rigidity of the os uteri, with dangerous haemorrhage.”*

No.	Complete or Partial Presentation.	Treatment.	Child.	Mother.
266	Not stated.	Turning.	Alive.	Died.
267	Not stated.	Extraction by foot.	Not stated.	Recovered§.
271	Complete.	Turning.	Not stated.	Died.
272	Partial.	Membranes ruptured.	Dead.	Died.
274	Partial.	Membranes ruptured.	Not stated.	Died.
277	Complete ?	Turning.	Alive.	Died.
282	Complete.	Extraction by feet.	Not stated.	Died.
283	Complete ?	Craniotomy.	Dead.	Died.
284	Complete.	Extraction by feet.	Dead.	Recovered.
285	Complete ?	Turning.	Not stated.	Died.
287	Complete.	Extraction by feet.	Not stated.	Recovered  .

\* Dr. Lee has here committed a statistical error in regard to the number of placental presentations occurring in his own practice, and reported in his Lectures. The number should be 38, and not 36.

† Another statistical mistake of Dr. Lee regarding the number of *his own* cases. His Clinical Midwifery contains 36 and not 35 cases of placental presentation. See other of Dr. Lee’s inadvertent errors on this head mentioned in a subsequent note respecting the number of children lost in these and other placental presentations.

‡ Probably the number 11 is indicative of another error in Dr. Lee’s reports. Dr. Lee, in his Lectures, adverts to 13 such cases; in his Clinical Midwifery, he limits the number to 11. If the cases were 13 in number, then the number 11 is wrong; or the reverse; for although he has reported 38 cases, in all, in his Lectures, and 36 in his

clinical work, yet neither of the two *additional* cases reported in the Lectures presented any difficulty on the part of the os uteri. In one case (Case 37) it was “little dilated but dilatable”; in the second (Case 38) the report is, “os uteri dilated to size of a crown-piece, dilatable.” If we admit 13 instead of 11 cases, we must, I believe, include Cases 260 and 289 of the Clinical Report. In both of these cases the mothers died. This would give us in the text a proportion of ten maternal deaths out of thirteen mothers operated on.

§ “A violent rigor (Dr. Lee states) followed [the delivery] which threatened for a time to destroy the patient. Bottles of hot water were applied to the feet and pit of the stomach, the whole body was covered with hot blankets, and brandy was freely administered. She slowly recovered from the effects of the immense loss of blood.”

|| “The pulse could scarcely be perceived for

Here we have only three mothers saved out of eleven operated upon; and two of the three saved evidently made a very narrow escape from death. I doubt if the most fatal of all human diseases—the plague itself—he found to destroy so large a proportion of those attacked. At all events, the operation of turning and artificial delivery, in unavoidable haemorrhage, with the os uteri imperfectly dilated, would, from these and other cases, appear to be more deadly than any operation that is deemed justifiable in the whole circle of surgery. It is more mortal even than Ovariotomy.

I believe, on the other hand, that in the above and similar cases, by the introduction of a finger, or of a common sound or bougie, (such as Dr. Hamilton employed when the os uteri was still shut, and in order to separate the membranes for some inches from the cervix\*, in order to induce premature labour), the placenta might be readily and completely detached—the attendant bleeding in this way arrested—and the labour subsequently allowed to proceed to a natural and safe termination, if it were a head or pelvic presentation. And if the child were placed transversely, a more safe and proper period could be waited for and selected for the version of it.

Would the strength of the natural organic adhesions of the placenta to the uterus prevent the easy separation in this way of the one organ from the other? I believe not. Speaking of the mere anatomical fact, Dr. William Hunter, in his celebrated work on the Gravid Uterus, observes, that the separation of the placenta from the uterus is “commonly practicable with the least imaginable force.” In his paper on the Structure of the Placenta, published in the Philosophical Transactions for 1832, Dr. Lee, whose intimacy with Dr. Hunter’s work is well known, curiously uses not only a similar, but exactly the same quaint expression and words, telling us that generally after labour the placenta is detached from the uterus “with the least imaginable force.”

*Thirdly.*—Dr. Lee argues against the practice of extracting the placenta before the child, because it was not followed by “Guillemeau, Mauriceau, Portal, Levret, Giffard, &c. &c.” If the argument were true, it would be one of no weight, because, on exactly the same ground, nothing novel should ever be allowed to be introduced into practice. Dr. Lee has fallen into some curious mistakes† in the two or three different

many hours after, but the circulation in the extremities was gradually restored, and she recovered.”

\* Dr. Lee himself seems to have met with no difficulty of any kind in following this practice. See Lectures, p. 319; and Clinical Midwifery, Cases 142, 145, &c.

† “We are solely (says Dr. Lee) indebted to Levret for the discovery of every important fact relating to the causes, the symptoms, and the

histories which he has attempted to give of placental presentations. I shall leave it to my professional brethren whether the following misrepresentation is to be referred to the same category of mistakes, or is capable of—a more direct and simple explanation.

Dr. Lee has given in his published “Lectures on the Theory and Practice of Midwifery” a special and detailed account of the individual cases of placental presentation recorded by Portal, and had therefore taken evidently very great pains to study minutely that author’s views and practice in this complication. In his late paper in the MEDICAL GAZETTE, Dr. Lee strongly asserts that Portal is one of those great practical accoucheurs who never “attempted in a single instance to tear away or detach the placenta from the neck of the uterus, when it was so undilatable as to render it impossible to pass the hand to turn the child and deliver, nor in any other condition whatever of the part, before the birth of the child.” p. 895.

In describing his 43d case, Portal observes “Je glissay ma main dans l’entrée de la matrice, où je sentis l’arriere-faix qui se presentoit. L’ayant séparé, afin de me frayer le chemin, je sentis les membranes des eaux que je perçay, et les eaux s’estant écoulées, je tiray l’arriere-faix le premier, afin qu’il ne m’incommodast point à la sortie de l’enfant.” Here Portal distinctly states that he separated and extracted the placenta first, and before trying to extract the child. He states the same thing in his 69th case, and, if possible, still more explicitly\*. Dr. Lee, who, on the present occasion, so strenuously asseverates that Portal did never, in a single instance, follow this practice, actually quoted and printed last year in his published Lectures, and from the French edition of Portal’s work the first half of the above sentence†, in which Portal himself so circumstantially declares that he did follow this practice. (See the quotation in Dr. Lee’s Lectures, p. 366.) I feel assured that any additional comment of mine will be here excused, as entirely superfluous.

*Fourthly.*—Dr. Lee states that the practice which I have ventured to recommend in placental presentations “was performed two hundred years ago by an ignorant and audacious impostor, on a lady who died in Paris, whose case is related, with denunciations of the practice, by Guillemeau.”

treatment of this (the unavoidable) variety of flooding in the latter months of gestation.” (Researches on the Diseases of Women, p. 209.) In his late Lectures, p. 368, Dr. Lee adduces a variety of evidence to shew that Levret on this point only “undertook to prove (to use again Dr. Lee’s own words) what, it appears, had previously been demonstrated”—by Portal, Mauriceau, Giffard, Smellie, &c.

\* Je separay tout doucement cet arrière-faix, et je tiray dehors; ensuite je glissay ma main dans la matrice, &c. &c.

† Dr. Lee’s quotation terminates at the word “perçay.”

It is a singular fact, and shows how differently two men *may* interpret an author's meaning, that in the discussion to which the proposed practice has given rise, Guillemeau should have been now twice brought up in evidence against me, in order to prove directly contrary allegations. In the Provincial Medical Journal for April, Dr. Blenkinsop published Guillemeau's rules of treatment in placental presentations, in order to show that Guillemeau had actually long ago recommended the artificial detachment of the placenta before the child. Now Dr. Lee appeals to Guillemeau's writings to show that Guillemeau actually long ago denounced the practice in question. I have elsewhere taken occasion to show that Dr. Blenkinsop's mistake was an inadvertent error of judgment. Dr. Lee's mistake consists in simply misrepresenting the facts of the case he alludes to. I recollect the results of Guillemeau's case well. In an instance of accidental (?) haemorrhage the midwife pulled at the ruptured membranes, and dragged away them and a *part* of the placenta. If she had separated the *entire* placenta, as has in ignorance been repeatedly but safely done by other midwives since her time, the flooding would in all probability have ceased. As it was, Guillemeau states that she separated only a *part* of the placenta, and consequently the mother almost inevitably died. Surely Dr. Lee understood Guillemeau so far as to know that it was hence an instance not at all in point, or hearing in any degree upon the subject, inasmuch as it was *in truth* not an instance of detachment of the whole placenta.

*Fifthly.*—Dr. Lee objects that the child would inevitably be lost by the mode of practice which I have described. The objection which has been often urged against my views is stronger in appearance than in reality. For, without insisting upon the principle generally acknowledged by Dr. Lee and other British accoucheurs, that we should sacrifice the child in those cases of extreme danger in which that sacrifice adds greatly to the chances of the safety on the part of the mother,—there are various other considerations, connected with the life of the child itself, which destroy the apparent force of the argument.

The fact is, that in cases of placental presentation treated under the present acknowledged rules of management, a very large proportion of the children are lost. I have previously stated that in his Clinical Midwifery Dr. Lee has detailed and reported thirty-six, and not, as he himself inadvertently but erroneously reckons them, thirty-five\* cases of unavoidable haemorrhage. In 13 out of these 36 cases Dr. Lee

does not state the result to the child, though the context leaves no doubt that in several of these cases the infant perished during delivery. In 23 cases he mentions the condition of the child at birth. In 8 out of these 23 cases it was born alive; in the remaining 15 it was born dead. Hence *nearly 2 out of every 3 of these children died* under this complication, or 65 per cent. of them were lost.

Besides, in exactly those varieties or complications of unavoidable haemorrhage in which I have ventured as yet to recommend the practice of detaching the placenta, the child is already in most instances inevitably lost, or almost certain to perish under any of the established modes of treatment; that is, it is either too weakly or premature to be viable, or it is almost sure to perish if forced delivery is attempted (as when the os uteri is imperfectly dilated or the pelvis contracted), or it is actually dying or dead when interference is required. On the other hand, the child is not always lost when the placenta is detached before it. Out of 106 cases in which the placenta was expelled before the child, and the result to the latter noted, the infant was born alive in 33 instances (see Dr. Cormack's Journal for March last); or 31 per cent. of the children were saved. In most of these cases the child was expelled within a few minutes after the complete separation of the placenta. When the interval is longer, and we require, after the detachment of the placenta, to wait for a length of time, is there no hope of making the child survive by continuing either its placental or pulmonary respiration during the intervening period? Dr. Lee tells us that in some cases of pelvic presentation, acting upon the suggestion of Dr. Bigelow and "older accoucheurs," he has, before the head could be extracted, pressed back the maternal parts "that the air may gain admission into the mouth of the child and the respiration go on, when the circulation in the cord has been arrested. I have seen (he adds) from twenty minutes to half an hour elapse in some cases after the cord had ceased to pulsate. . . . If the head he low down, the fingers alone can give the necessary assistance; but if it is high in the pelvis, and reached with difficulty, the assis-

ters which he has affixed to his cases in the table printed at p. 893 of his paper. In his Clinical Midwifery, in reporting his placental cases, he has committed another numerical mistake in passing from Case 289 to Case 291, omitting altogether 290. I mention these mistakes as liable to mislead us in some calculations, and not with the view of showing any desire to impute blame or offer serious criticism for errors of such a caste, and which it is so difficult always to avoid. In the number of the GAZETTE containing Dr. Lee's late paper, the Editor has shown (p. 917) that the Registrar-General himself, whose very profession consists of statistical calculations, has published a very "serious error" of a numerical kind, in one of his late official returns.

\* In his late paper in the GAZETTE, Dr. Lee commits the same mistake in summing up the number of his own cases of placental presentation. Hence, there is an error in all the eight num-

tance of a tube may be required." (Lectures, p. 335.) Is it hopeless to suppose that the same principle, or other means, may yet be successfully employed to keep the child alive, after the placenta is extracted in unavoidable hæmorrhage, and in some cases give it even a greater chance of life than under the continuance of the flooding, or the operation of forced delivery?

*Lastly.*—Dr. Lee seems to believe that one of my tables gives an erroneous view of the common degree of maternal danger attendant upon placental presentations, when it shows that about 1 out of every 3 mothers perishes under this obstetric complication.

Some years ago Dr. Churchill endeavoured to ascertain statistically the number of mothers that died under placental presentations, and from a variety of data calculated that the mortality amounted to about 1 in 3.

In his own Lectures on Midwifery, published in 1844, Dr. Lee quotes, and so far adopts from Dr. Churchill, the fact that "out of 174 cases of placental presentation recorded by different authors, 48 proved fatal, or nearly 1 in 3." (Dr. Lee's Lectures, p. 371).

We have already seen that in Dr. Lee's own recorded cases of turning in unavoidable hæmorrhage, the maternal mortality was greater than 1 in 3.

The table of maternal deaths printed in my essay in Dr. Cormack's Journal, is in perfect accordance with these results of Dr. Churchill and Dr. Lee. The principal difference is, that it contains a much larger number and more extensive foundation of statistical data. In collecting its materials I proceeded rigidly upon the principle of only entering upon it the results of the practice of those individuals or institutions upon whose records I could find ten or more cases of unavoidable hæmorrhage. In this way I believed I would be more certain to arrive at an accurate statistical result, than if I made my calculation upon the collection of cases of a smaller number scattered throughout our medical journals. I noted down all the lists of instances I could detect in which ten or more cases were reported. Latterly, I have found that I erroneously omitted Paul Portal, because I relied on Dr. Lee's accuracy, when, in his Clinical Mid-

wifery, he stated that Portal's work contained an account of "eight" cases only of unavoidable hæmorrhage, while it contains notices of the results of fourteen. In drawing up the table I am not at all further ashamed to own, that, harassed as I was at the time with abundance of other professional occupation, I fell, in working up the data, into some other inadvertent errors, which will be found rectified in an extended essay on the whole subject, the printing of which is now nearly completed. Seeing that Dr. Lee and Dr. Ramsbotham have both committed numerical errors of the same kind in summing up, and calculating upon, the results of *their own* limited number of placental cases, it will perhaps be considered the more excusable that in searching out and reckoning up the results of far more numerous returns and reports of a similar description given by others, and, for the most part, scattered in a disjointed and unarranged form throughout their published works, I should have committed some similar errors. I did not, for example, discover some additional instances of death of the mother in placental presentation in Smellie's works, in a section (where I did not expect them) upon the Cæsarean operation; and I have corrected one or two errors of the same kind by the more careful collation of the writings of Giffard, &c. But these corrections do not alter, in any practical degree, the *statistical result* regarding the degree of mortality among mothers in placental presentations. More extensive data than I had access to may alter that result, but probably not to any marked amount. And, indeed, the actual total fatality of the complication may possibly be even higher than such calculations can prove, because they demonstrate the consequences of the complication and its treatment in the hands of the highest members of the profession, while they afford us little or no insight into the number of deaths produced by it among the patients of less experienced practitioners. As to the special mistake in my table of cases which called forth the animadversions of Dr. Lee in his late paper in the *GAZETTE*, I beg, in exculpation, to submit to the readers of that journal, some letters, explanatory of its nature, and illustrative of the difficulties attendant upon all such inquiries.